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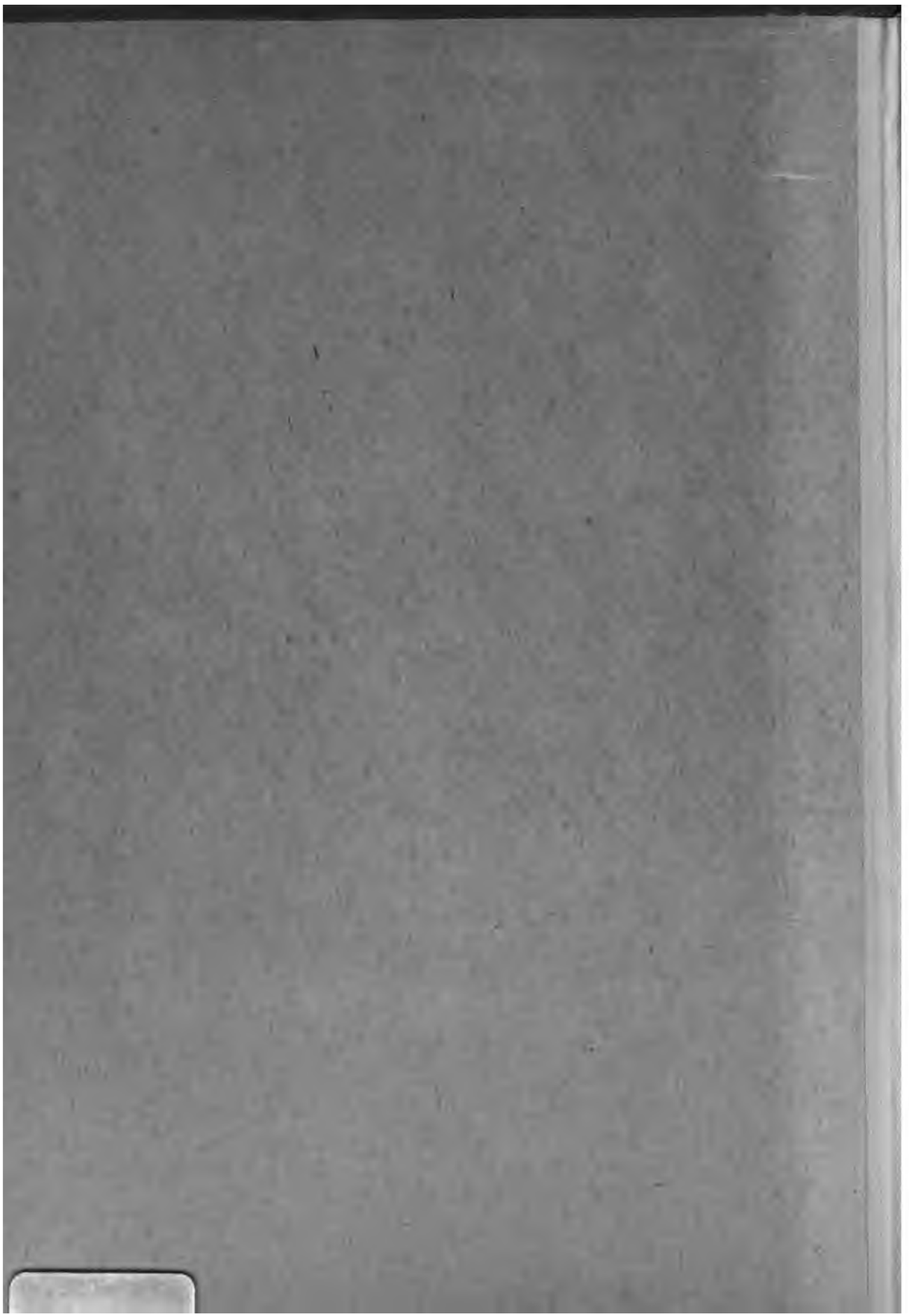

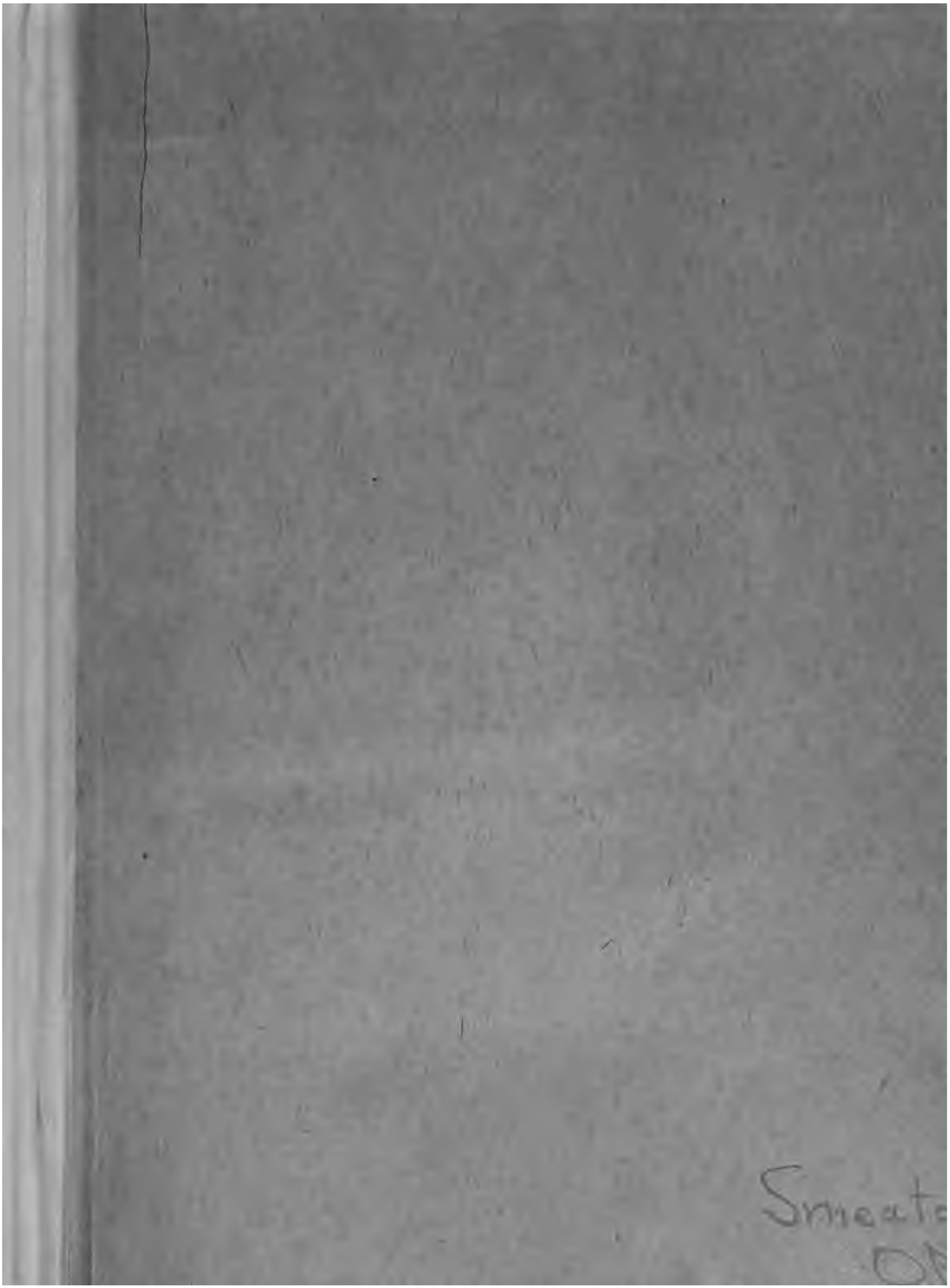

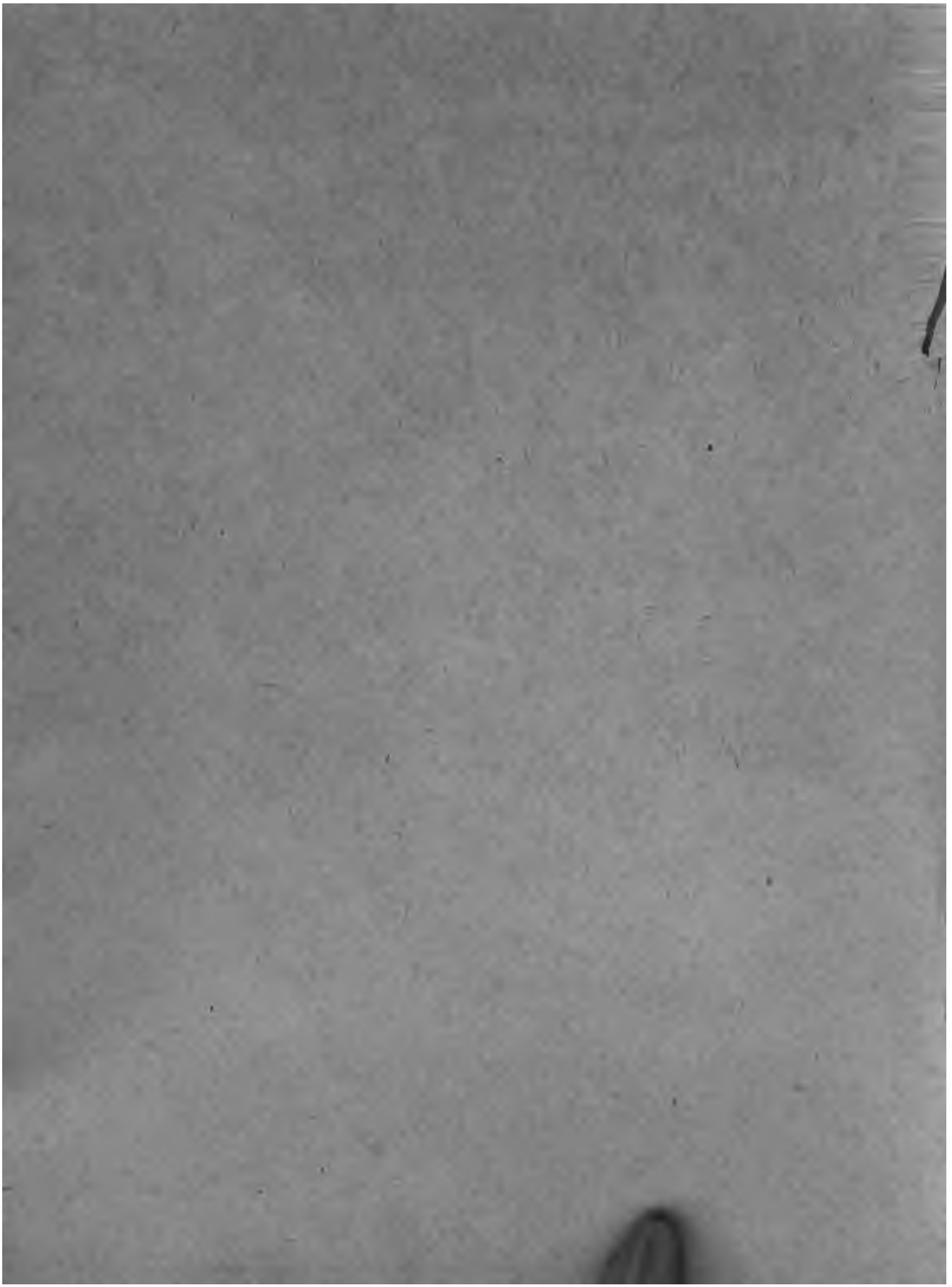

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THE papers which are here collected together include the whole of Mr. SMEATON's communications to the Royal Society, published in the Philosophical Transactions. Most of the articles have been reprinted verbatim from the originals in that work: a few of them have been printed from copies which had belonged to the author, and contained corrections and additions in his own hand writing. For the latter the editor has to make his acknowledgments to Mr. SMEATON's daughter, Mrs. DIXON, who obligingly lent him the tracts.

The accompanying plates, with one or two exceptions, are the same as were originally given with the several articles in the Philosophical Transactions; the President and Council of the Royal Society having, in the handsomest manner, granted the loan of the coppers, in order to afford every facility in their power to the publication.

The present volume completes the series of Mr. SMEATON's works. The account of the Edystone light-house was published during his life-time, and has been lately reprinted. Since his death, the Committee of Civil Engineers have published, with the liberal permission of Sir JOSEPH BANKS, who had purchased the whole of the manuscripts, three volumes of Mr. SMEATON's Reports, which comprise every paper of

of any conſequence compoſed by him, in the way of his profeſſion, during the long period of his public life. To theſe volumes, the preſent may be conſidered as an important and indiſpenſable appendage; and the whole together will be found to contain a moſt valuable fund of practical inſtruction in the ſeveral branches of Science and the Arts, which had occupied the attention of the ingenious author, and upon which he has raiſed for himſelf a high and laſting reputation.

London, July 20th, 1814.

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TREATISES

AND

MISCELLANEOUS PAPERS.

[*Philos. Transact.* Vol. XLVII. Article 69.]

A LETTER from Mr. John Smeaton to Mr. John Ellicott, F. R. S. concerning some Improvements made by himself in the Air-Pump.

SIR,

Read April 16, 1752. I HAVE been informed by ſome of my friends, that my endeavours towards completing the air-pump, have been mentioned with approbation, in papers that Mr. Short and Mr. Watſon have lately communicated to the Royal Society. I underſtand likewiſe, that the latter of thoſe gentlemen has, in a very obliging manner, expreſſed a wiſh, that I ſhould lay before the Society a particular account of my improvements therein.

I ſhall always eſteem it a ſingular honour to be thought capable of producing any thing worthy the attention of the Royal Society; and to be my duty and intereſt ſo to do, upon the leaſt intimation of that kind.

Your ſuperior ſkill in mechanics, together with the aſſiſtance you have given me in making trial of my pump, againſt three very good ones of the common conſtruction, as well as the frequent marks of friendſhip you have ſhewn me on all occaſions, encourage me to trouble you with communicating the following to that Society, of which you are a member, and who, of all others, are the moſt proper judges.

Smeets
OAL

But ſuppoſing all thoſe difficulties to be abſolutely overcome, the other defect that I mentioned in the common conſtruction, would hinder the rarefaction from being carried on beyond a certain degree. For, as the piſton cannot be made to fit ſo cloſe to the bottom of the barrel, as totally to exclude all the air; as the piſton riſes, this air will expand itſelf; but ſtill preſſing upon the valve, according to its denſity, hinders the air within the receiver from coming out: hence, were this vacancy to equal the 150th part of the capacity of the whole barrel, no air could ever paſs out of the receiver, when expanded 150 times, though the piſton were conſtantly drawn to the top; becauſe the air in the barrel, when in its moſt expanded ſtate, would be *in æquilibrio* with that in the receiver. This obſtacle I have endeavoured to overcome, by ſhutting up the top of the barrel with a plate, having in the middle a collar of leathers, through which the cylindrical rod works, that carries the piſton. By this means, the external air is prevented from preſſing upon the piſton; but that the air, that paſſes through the valve of the piſton from below, may be diſcharged out of the barrel, there is a valve applied to the plate at the top, that opens upwards. The conſequence of this conſtruction is, that when the piſton is put down to the bottom of the cylinder, the air in the lodgement under the piſton will evacuate itſelf ſo much the more, as the valve of the piſton opens more eaſily, when preſſed by the rarefied air above it, than when preſſed by the whole weight of the atmoſphere. Hence, as the piſton may be made to fit as nearly to the top of the cylinder, as it can to the bottom, the air may be rarefied as much above the piſton as it could before have been in the receiver. It follows therefore, that the air may now be rarefied in the receiver, in duplicate proportion of what it could be upon the common principle; ſuppoſing other impediments out of the queſtion.

Another advantage of this conſtruction is, that though the pump is compoſed of a ſingle barrel *, yet the preſſure of the outward air being taken off by the upper plate, the piſton is worked with more eaſe † than the common pumps with two barrels: and not only ſo, but when a conſiderable degree of rarefaction is deſired, it will do it

* It is obvious that these improvements will equally obtain, whether the pump be constructed with a single or a double barrel.

† Because, though the pressure of a column of air, equal to the diameter of the piston-rod, still presses upon it, yet, as there is only the friction of one piston, and that not loaded with the weight of the atmosphere, the friction of the leather against the side of the barrel, and that of the rack and wheel, is much less: so that, notwithstanding the addition of friction in the collar of leathers, that of the whole will be less.

quicker;

quicker; for the terms of the ſeries expreſſing the quantity of air taken away at each ſtroke do not diminiſh ſo faſt, as the ſeries anſwering to the common one.

I have found the gages that have been hitherto made uſe of, for meaſuring the expanſion of the air, very unfit to determine in an experiment of ſo much nicety. I have therefore contrived one of a different ſort, which meaſures the expanſion with certainty, to much leſs than 1000th part of the whole. It conſiſts of a bulb of glaſs, ſomething in the ſhape of a pear, and ſufficient to hold about half a pound of quick-ſilver. It is opened at one end, and the other is terminated by a tube hermetically cloſed at top. By the help of a nice pair of ſcales, I found what proportion of weight a column of mercury, of a certain length, contained in the tube, bore to that, which filled the whole veſſel. By theſe means I was enabled to mark diviſions upon the tube, anſwering to a 1000th part of the whole capacity, which being of about one tenth of an inch each, may, by eſtimation, be eaſily ſubdivided into ſmaller parts. This gage, during the exhauſting of the receiver, is ſuſpended therein by a ſlip-wire. When the pump is worked as much as ſhall be thought neceſſary, the gage is puſhed down, till the open end is immerged in a ciſtern of quickſilver placed underneath: the air being then let in gradually, the quickſilver will be driven into the gage *; till the air remaining in it becomes of the ſame denſity with the external; and as the air always takes the higheſt place, the tube being uppermoſt, the expanſion will be determined by the number of diviſions, or parts of a diviſion, occupied by the air at the top.

The degree, to which I have been able to rarefy the air in experiment, has generally been about 1000 times, when the pump is put clean together: but the moiſture, that adheres to the inſide of the barrel, as well as other internal parts, upon letting in the air, is in ſucceeding trials worked together with the oil, which ſoon renders it ſo clammy, as, being minutely divided, to obſtruct the action of the pump upon a fluid ſo ſubtil as the air is, when ſo much expanded; but in this caſe it ſeldom fails to act upon the air in the receiver, till it is expanded 500 times: and this I have found it to do, after being frequently uſed for ſeveral months, without cleaning. I have alſo generally found it to perform beſt, the firſt trial at each time of uſing; though nothing had been done at it from the time preceding; which, after a great many trials made with

* The bulb of the gage may be emptied of its quickſilver, without emptying the tube; and afterwards the tube being held horizontally, the column of mercury in it will have no power to contract or expand the air at the top.

this

this view, I alſo attribute to the watry vapours of the air mixing with the oil. An experiment, where the air was expanded 1000 times, was tried about two years ſince in your preſence; at which were preſent alſo Dr. Knight and Mr. Canton; and I lately did the ſame thing with Mr. Watſon *. The pump, which I have the honour of ſhewing the Society, is the ſame that I juſt now mentioned, and the ſecond that I made, with a view to improve upon this principle.

The degree of rarefaction, produced by the beſt of the three pumps, that you procured the trial of, and which you eſteemed good in their kind, and in complete order, never exceeded 140 times, when tried by the gage above deſcribed †.

I have alſo endeavoured to render the pneumatic apparatus more ſimple and commodious, by making this air-pump act as a condenſing engine at pleaſure, by ſingly turning a cock. This not only enables us to try any experiments under different circumſtances of preſſure, without changing the apparatus, but renders the pump an univerſal engine, for ſhewing any effect, that ariſes from an alteration in the denſity or ſpring of the air. Thus, with a little addition of apparatus, it ſhews the experiments of the air-fountain, wind-gun, &c.

The condenſing part is contrived in the following manner: The air above the piſton being forcibly driven out of the barrel at each ſtroke, and having no where to eſcape, but by the valve at the top; if this valve be connected with the receiver, by means of a pipe, and at the ſame time the valve at the bottom, inſtead of communicating with the receiver, be made to communicate with the external air, the pump will then perform as a condenſer.

The mechaniſm is thus ordered. There is a cock with three pipes placed round it, at equal angles. The key is ſo pierced, that any two may be made to commu-

* Though an expansion of 1000 times is mentioned here, as capable of being procured at pleasure; yet this is by no means the utmost ever arrived at: for when perfect tightness has concurred with perfect cleanness, and perhaps some other causes, that the author does not profess to be acquainted with, it has made a rarefaction of 2000, 5000, and even 10,000 times, a degree of rarefaction seldom to be obtained in the *Torricellian* tube.

† *Boyle*, *Desaguliers*, and others, make mention of rarefying the air by their pumps 14,000 times and upwards: but as the experiment was made by expending a whole tube of air upon the surface of water, which is capable of emitting a quantity of air when under the exhausted receiver of the common air-pump, no certain conclusion can be drawn therefrom; on the contrary, it appears from experience, that a common air-pump that will, with certainty, rarefy the air 100 times, is not to be accounted a bad one.

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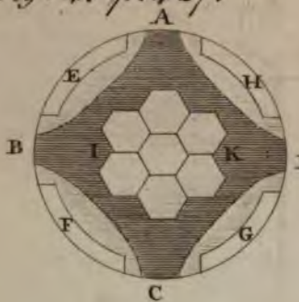
Fig. 4. p. 427.



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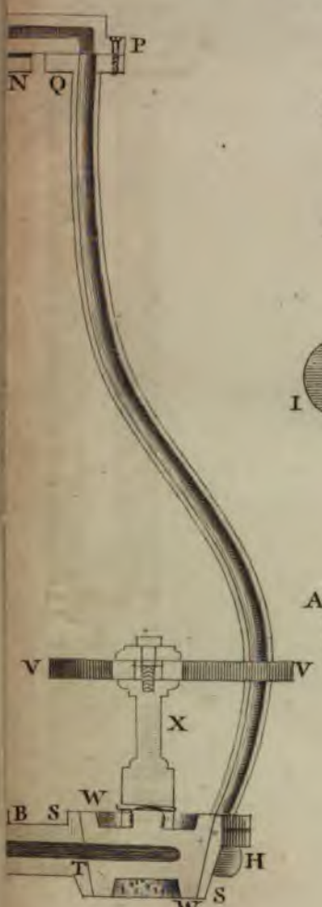
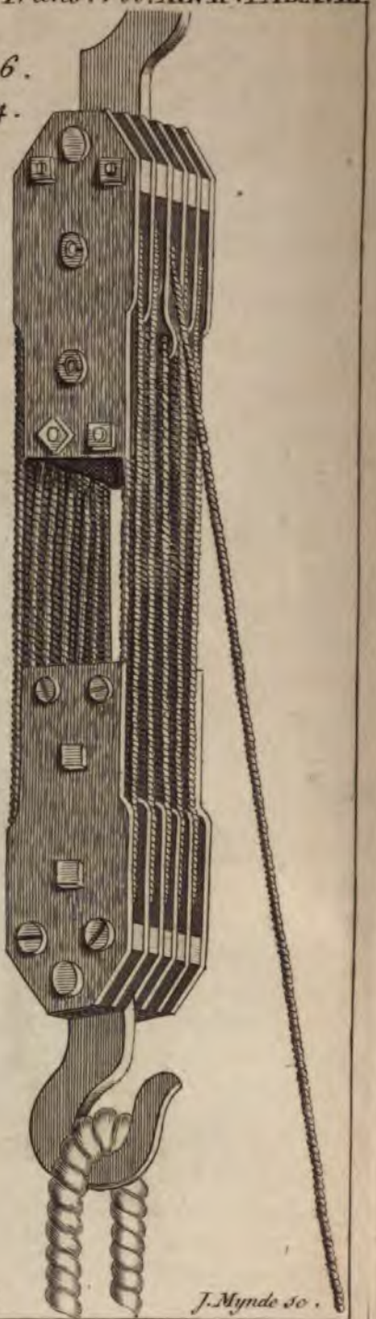


Fig. 5.
p. 428.

Fig. 3.
p. 426.

Fig. 6.
p. 494.



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For Fig. 4.

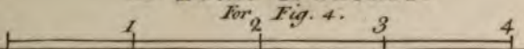

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Plate 1. page 7.

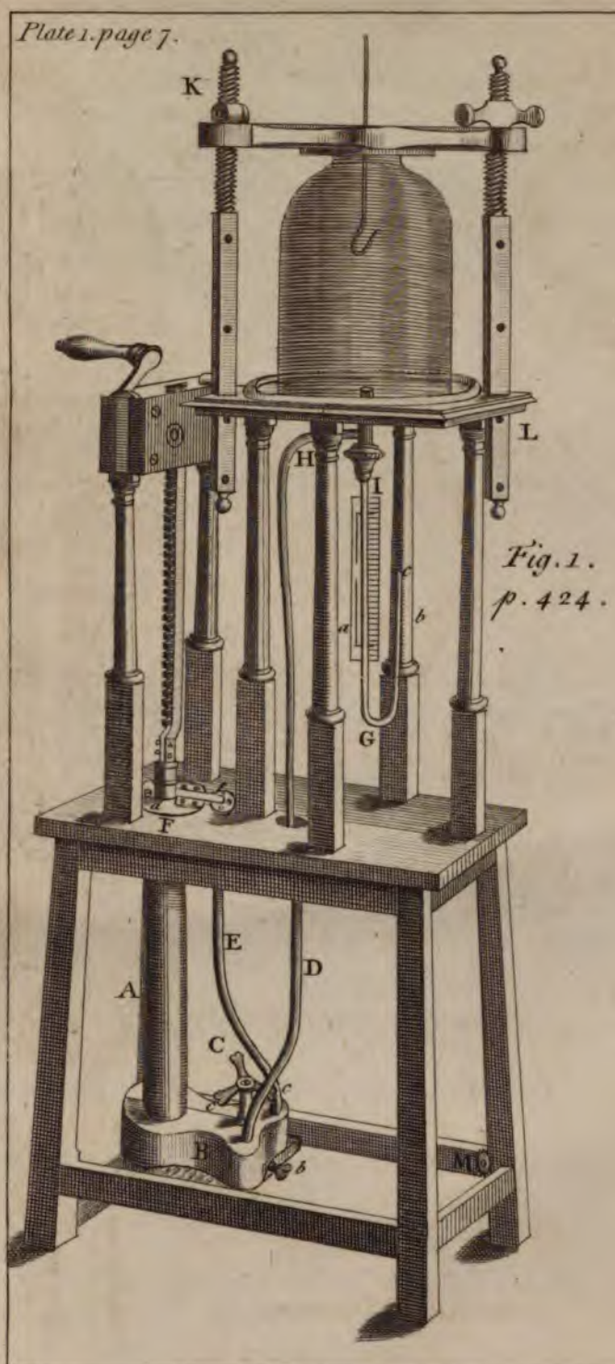

Fig. 1. p. 424.

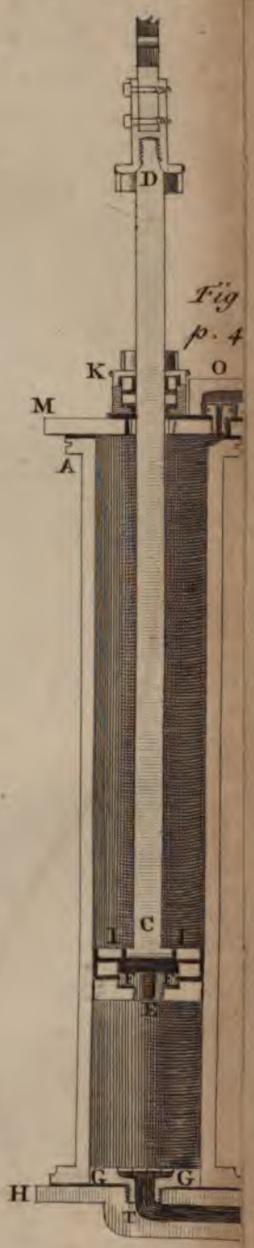

Fig p. 4

2.

5.

nicate, while the other is left open to the external air. One of theſe pipes goes to the valve at the bottom of the barrel; another communicates with the valve at the top, and a third with the receiver. Thus, when the pipe from the receiver, and that from the bottom of the barrel, are united, the pump exhauſts: but turn the cock round, till the pipe from the receiver, and that from the top of the barrel, communicate, and it then condenſes. The third pipe, in one caſe, diſcharges the air taken from the receiver, into the barrel; and in the other, lets it into the barrel, that it may be forced into the receiver.

I am,

SIR,

Your moſt humble ſervant,

J. SMEATON.

Furnival's-Inn Court,
April 16, 1752.

P. S. I have alſo added ſome draughts, and letters of reference, in order to explain myſelf more fully.

Plate 1. Figure 1.

Is a perſpective view of the principal parts of the pump together.

A is the barrel.

B the ciſtern, in which are included the bottom of the barrel and cock, with three communications. Theſe are covered with water to keep them air-tight. A little cock to let the water out of the ciſtern, is marked *b*.

C *c c* is the triangular handle of the key of the cock: which, by the marks on its arms, ſhews how it muſt be turned, that the pump may produce the effect deſired.

D H is the pipe of communication between the cock and the receiver.

E is the pipe, that communicates between the cock and the valve, on the upper plate of the barrel.

F is the upper plate of the pump, which contains the collar of leathers *d*, and V, the valve, which is covered by the piece *f*.

G I is the ſiphon-gage; which ſcrews on and off, and is adapted to common purpoſes. It conſiſts of a glaſs tube, hermetically ſealed at *c*, and furniſhed with quickſilver in each leg; which, before the pump begins to work, lies level in the line *a b*; the ſpace *b c* being filled with air of the common denſity. When the pump exhauſts, the air in *b c* expands, and the quickſilver in the oppoſite leg riſes, till it becomes a counter-balance to it. Its riſe is ſhewn upon the ſcale I *a*, by which the expanſion of the air in the receiver may be nearly judged of. When the pump condenſes, the quickſilver riſes in the other leg, and the degree may be nearly judged of, by the contraction of the air in *b c*: marks being placed at $\frac{1}{2}$ and $\frac{1}{3}$ of the length of *b c* from *c*; will ſhew when the receiver contains double or treble its common quantity.

K L is a ſcrew-frame to hold down the receiver, in condenſing experiments, which takes off at pleaſure; and is ſufficient to hold down a receiver, the diameter of whoſe baſe is ſeven inches, when charged with a treble atmoſphere: in which caſe it acts with a force of about 1200 pounds againſt the ſcrew-frame.

M is a ſcrew that faſtens a bolt, which ſlides up and down in that leg, by means whereof the machine is made to ſtand faſt on uneven ground.

Fig. 2.

Is a perpendicular ſection of the barrel and cock, &c. where

A B repreſents the barrel.

C D the rod of the piſton, which paſſes through

M N the plate, which cloſes the top of the barrel.

K is the collar of leathers, through which the piſton-rod paſſes. When the piſton is at the bottom of the cylinder, the upper part of K is covered by the cap at D, to keep out duſt, &c.

The valve on the upper plate, is covered by the piece

O P, which is connected with the pipe

Q H, which makes the communication between the valve and cock.

C E

C E is the piſton; and

E F F is the piſton-valve.

I I are two little holes to let the air paſs from the piſton-valve into the upper part of the barrel.

G G T is the principal valve at the bottom of the cylinder.

H H is a piece of metal, into which the valve G G T is ſcrewed, and cloſes the bottom of the cylinder; out of which alſo is compoſed

S S the cock, and

T T the duct from the cock to the bottom of the barrel.

W W is the key of the cock.

X the ſtem; and

V V the handle.

Fig. 3.

Is an horizontal ſection of the cock, through the middle of the duct T T.

A B repreſents the bigneſs of the circular plate, that cloſes the bottom of the barrel.

C D repreſents the bigneſs of the inſide of the barrel.

E F G is the body of the cock; the outward ſhell being pierced with three holes, according to an equilateral triangle, and correſponding to the three ducts L H, N I, M K, whereof

L H is the duct, that goes to the bottom of the barrel.

N I, the duct, that communicates with the top of the barrel; and

I I the flanch or base for the joint with the pipe E, in fig. 1.

M K is the duct, that passes from the cock to the receiver; and

K K the flanch for the joint, with the pipe D, in fig. 1.

L M N is the key, or solid part of the cock, moveable round in the shell E F G. When the canal L M answers to the ducts H and K, the pump exhausts, and the air is discharged upwards by the perforation N. But the key L M N being turned till the canal L M answers to I and K, the perforation N will then answer to H H; and in this case the pump condenses. Lastly, when N answers to K K, the air is then let in or discharged from the receiver, as the circumstance requires.

Fig. 4.

Is the plan of the principal valve.

A B C D represents the bladder fastened in four places, and stretched over the seven holes I K, formed into an hexagonal grating; which may be called the honeycomb.

E F G H shews where the metal is a little protuberant, to hinder the piston from striking against the bladder.

Fig. 5.

Represents the new gage; which, from its similitude, may be called the pear-gage. It is open at A; B C is the graduated tube, which is hermetically closed at C, and is suspended by the piece of brass D E, that is hollowed into part of a cylinder, and clasps the tube.

AN

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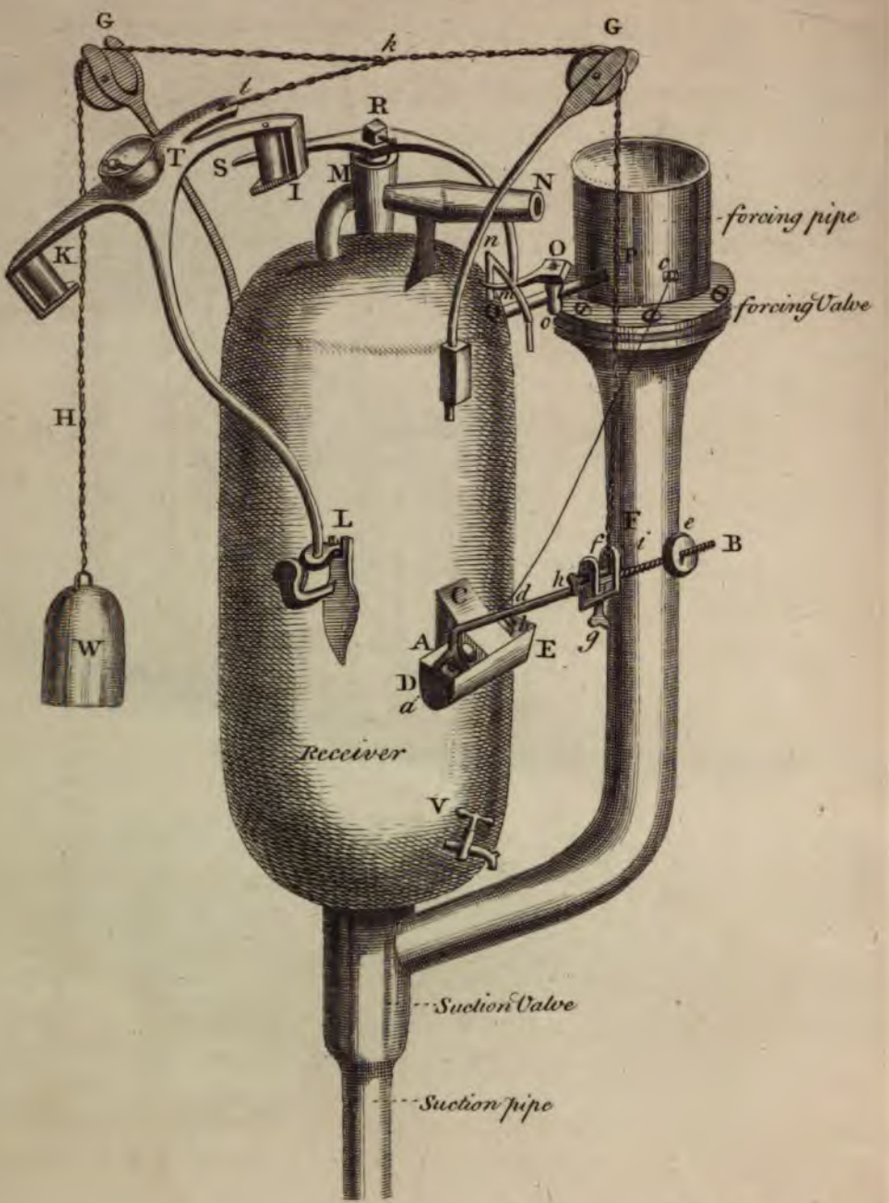
Philos. Trans. Vol. XLVII. **TAB.** XIX. *p.* 437. *Plate* 2. *page* 11.



J. Mynde sc.

AN ENGINE for raising Water by Fire, being an Improvement of SAVERY's Construction, to render it capable of working itself, invented by Mr. DE MOURA, of Portugal, F. R. S. described by Mr. J. SMEATON.

Read Nov. 9, 1752. THIS engine conſiſts of a receiver, a ſteam, and an injection cock, a ſuction and a forcing pipe, each furniſhed with a valve, together with a boiler, which, on account of its bulk and weight, is not ſent with the reſt; but, as it may be of the common globular ſhape, and having nothing particular in its conſtruction, a deſcription of it will not be neceſſary, as alſo the reſt of theſe parts already mentioned being eſſential to every machine of this kind, a further account of them may be diſpenſed with. What is peculiar to this engine is a float within the receiver, compoſed of a light ball of copper, which is not looſe therein, but faſtened to the end of an arm, which is made to riſe and fall by the float, while the other end of the arm is faſtened to an axis, and, conſequently, as the float moves up and down, the axis is turned round one way or the other. This axis is made conical, and paſſes through a conical ſocket, which laſt is ſoldered to the ſide of the receiver. Upon one of the ends of the axis, which projects beyond the ſocket, is fitted a ſecond arm, which is alſo moved backwards and forwards by the axis, as the float riſes or falls. By theſe means, the riſing or falling of the ſurface of the water within the receiver, communicates a correſpondent motion to the outſide, in order to give proper motions to the reſt of the geer; which regulates the opening and ſhutting of the ſteam and injection cocks, and ſerves the ſame purpoſe as the plug frame, &c. in Newcomen's engine. The particular conſtruction and relation of thoſe pieces will better appear by the figure and references, than can be done by a general deſcription.

Plate 2.

A B an arm, which is faſtened to

a b, a conical axis, which goes through a conical ſocket in

C, a triangular piece ſoldered to the receiver. This piece has this ſhape, to give liberty to the arm to riſe and fall, that carries the float on the inſide.

D E is a ſmall ciſtern, ſoldered to the receiver, which, being kept full of water, keeps the axis and ſocket air-tight. This ciſtern is conſtantly kept full of water by means

means of a ſmall leakage through the wooden peg *c*, which follows the packthread *c d* to the ciſtern.

e, is a ſmall weight to counterpoiſe the float within.

f, is a ſlider; which being ſet nearer to or farther from the axis, will riſe or fall, a greater or leſſer ſpace as may be required, and is faſtened by the ſcrew *g*. This ſlider is furniſhed with a turn about, *h i*, which is alſo faſtened by a ſcrew and nut at the end *i*, and ſerves to adjuſt the length of

F G G H, a chain, which gives motion, by means of the ſhorter chain *k l*, to

I K L, the balance which opens and ſhuts the cocks; and moves upon the ſmall axis L.

G G are two pullies, ſupported by two arms, that are faſtened to the ſide of the receiver, and give the chain a proper direction in order to move the balance.

M N is the ſteam cock, the end N being ſuppoſed to be detached from a pipe, that gives it communication with the boiler.

O is the injection cock, whoſe key is turned by the arm O *m*.

P Q is the injection pipe, communicating between the forcing pipe above the valve, and the top of the receiver.

R S is the arm, by which the key of the ſteam-cock is worked.

I K, two rollers annexed to the balance, which, by ſtriking upon the arm R S, open and ſhut the ſteam cock, as the balance is moved backward and forward.

R *n o* is the ſteam cock's key tail, which is furniſhed with two ſmall rollers, *n o*, which open and ſhut the injection cock, by acting upon the arm O *m*, in ſuch a manner, that when the ſteam cock is opened, the injection is ſhut, and *vice verſâ*.

T is a bell of advice, which, moving along with the balance, continues to ring as long as the engine is at work.

V is

V is a cock, which ſerves to diſcharge the air from the receiver, and is opened by hand when neceſſary.

W is a weight ſufficient to raiſe the balance to a perpendicular poſture, when it is inclined to the right, and alſo to overcome the friction of the float, axis, pullies, chain, &c.

To put the engine in motion, preſs down the arm A B, which will bring the balance over to the right ſide, and its motion will open the ſteam cock, and ſhut the injection; ſet open the cock at V, that the air may be diſcharged by the entrance of the ſteam into the receiver. This being done, ſhut that cock, and let go the arm; the weight W will bring over the balance to the left, and in its motion ſhut the ſteam cock, and open the injection; the preſently condenſing the ſteam into water, in a great meaſure leaves a vacuum in the receiver. Things remain in this ſituation, till the preſſure of the atmoſphere has cauſed the water to mount through the ſuction pipe into the receiver, where, as its ſurface riſes, it cauſes the float to aſcend; and depreſſing the arm A B, raiſes the balance, till it has paſſed the perpendicular; and, in its deſcent, which is done by its own gravity, the roller K lays hold of the arm R S, again opens the ſteam cock, and ſhuts the injection. The receiver being now almoſt filled with water, the balance cannot return till the ſurface of the water therein ſubſides, and ſuffers the float to deſcend. This is performed by the elaſticity of the ſteam, which, at the ſame time that it fills the receiver, drives out the water through the forcing pipe, and when the ſurface is deſcended ſo low, as to ſuffer the weight W to bring the balance beyond the perpendicular towards the left, it then falls of its own accord, and, in falling, the roller I, lays hold of the arm R S, ſhuts the ſteam cock and opens the injection, as before.

When the engine is deſired to be ſtopped, obſerve, when the balance lies to the right, to turn round the arm O *m* of the injection cock, ſo that the tail of the ſteam cock may miſs it in the next motion; ſo that, at the ſame time that the receiver is filled with ſteam, and the ſteam cock ſhut, the injection not being opened, the motion will ſtop for want thereof.

AN

AN Account of some Experiments upon a Machine for measuring the Way of a SHIP at Sea.

IN the *Philosophical Transactions*, No. 391, for November 1725, Mr. Henry de Saumarez, gives an account of a machine for meaſuring a ſhip's way more exactly than by the log. This machine conſiſts of a firſt mover, in the form of the letter Y. Upon the two arms of the Y are faſtened two vanes, inclined in ſuch a manner, that when the Y is hauled through the water by a rope, faſtened to the ſtem or tail thereof, it may turn round, and, of conſequence, endeavour to turn the rope round. The other end of the rope being faſtened to the end of a ſpindle capable of moving freely round, will be made to do ſo by the rotations of the Y, communicated to the rope. A motion being thus communicated to a ſpindle within the ſhip, this ſpindle may be made to drive a ſet of wheel-work, which will regiſter the turns of the Y; and the value of a certain number of theſe turns being once found, by proper experiments, they are eaſily reducible into leagues and degrees, &c. The only difficulty then is, whether this Y will make the ſame number of rotations in going the ſame ſpace, when it is carried through the water faſt, as when it is carried ſlow. Upon this head Mr. de Saumarez, as well in the paper above cited, as in a ſubſequent one publiſhed in the *Philosophical Transactions*, No. 408, for March 1729, has given an account of ſeveral trials, which he has made of it, from which it appears, that this machine, in part, anſwers the end propoſed, and is, in part, defective: the errors of which he ſuppoſes to proceed from the ſinking down of the Y into the water, upon a ſlow motion; the axis of its rotation being then more oblique to the horizon than in a quick one.

In a machine, conſtructed like this, it is evident, that the end of the ſpindle, to which the rope is faſtened, muſt be of ſufficient ſtrength and thickneſs, not only to bear the force or ſtreſs, that the hauling of the Y through the water will lay upon it, in the greateſt motion of a ſhip, but alſo to bear the accidental jerks, that the waves will ſuperadd thereto. The thickneſs of the ſpindle then being determined by theſe conditions; it is alſo manifeſt, that, to prevent the ſpindle from being pulled out of its place by the draft of the rope, there muſt be a ſhoulder formed upon it, which muſt be greater than the part of the ſpindle before deſcribed, for the ſpindle to bear againſt. The ſize, that Mr. Saumarez propoſes to give to his Y, is twenty-ſeven inches

inches the whole length; fifteen inches for the length of the arms (which are to be opened to a right angle); eight inches for the length of each vane; four inches and a half broad, and the ſtems and ſhank to be two-thirds of an inch thick. According to theſe dimenſions, the reſiſtance, that this part of the machine will meet with, in paſſing through the water, will, in the ſwift motions of the ſhip, be very conſiderable: conſequently, the neceſſary bulk of the pivot-end of the ſpindle, and its ſhoulder, will occaſion a conſiderable friction in the turning thereof, and retardation to the rotation of the machine.

To cure theſe defects as much as poſſible, inſtead of the Y before deſcribed, I made trial of a ſingle plate of braſs, of about ten inches long, two and an half broad, one-thirtieth of an inch thick, and cut into an oval ſhape. This plate being ſet a little a-twiſt, and faſtened by one end to a ſmall cord, in the manner of the Y, is likewiſe capable of making a rotation, in being drawn through the water; but with this difference, that as this is but a ſmall thin plate drawn edgeways through the water, its reſiſtance in paſſing through it is much leſs; of conſequence, a much ſmaller line is ſufficient to hold it, which again conſiderably diminiſhes the reſiſtance; and this, of courſe, proves a double diminution of friction in the ſpindle: Firſt, as the preſſure upon it is leſs; and, ſecondly, as it allows the ſpindle and ſhoulder to be of a leſs diameter. To break the jerks of the waves; next to the end of the ſpindle I fixed a ſpiral ſpring of wire, to which the cord was faſtened; which, by this means, was capable of playing backwards and forwards, and giving way to the irregularities of the ſea: and, leſt the plate ſhould lay faſt hold of any thing, or any extraordinary jerk ſhould damage the ſpindle or ſpring, a knob, or button, was faſtened upon the cord, at a ſmall diſtance from the ſpring, which ſtopped upon a hole in a piece of wood, and prevented the ſpring from being pulled out to above a certain length; ſo that all addition of force, beyond this, could only tend to break the cord, and carry away the plate. The ſpindle, being thus guarded from accidents, will allow of a ſtill further diminution of its ſize; ſo that, at laſt, I ventured to make the ſpindle-pivot no more than one-twentieth of an inch diameter, and that of the ſhoulder one-eighth; being of tempered ſteel, and ſufficiently ſmooth. The hole, in which the pivot, and againſt which the ſhoulder worked, was of agate likewiſe, well poliſhed.

Being thus provided, in May 1751, I procured a boat, upon the ſerpentine river in Hyde-park, to try how far the turns of the machine would be conſiſtent with themſelves, when the ſame ſpace was meaſured over with the ſame, and with different velocities.

ſuppoſe, that they proceeded from a retardation of the plate in ſwift motions, but from the hauling home of the log in ſlow ones. As for inſtance; the log, to do its office accurately, ought to remain at reſt in the water, whatever be the motion of the veſſel. But even the keeping the line ſtrait, and much more the ſuffering the log to haul the line off the reel (as practiſed by many,) will make the log, in ſome meaſure, follow the veſſel, and will be greater, in proportion as the time of continuance of this action is greater; and therefore the log will follow the ſhip twice as far in going one knot, when the ſhip is twice as long in running it. The conſequence of this is, that a veſſel always runs over a greater ſpace than is ſhewn by the log-line; but that this error is greater, in proportion as the veſſel moves ſlower. It is this reaſon, I ſuppoſe, that has induced the practical ſeamen to continue the diſtance between their knots ſhorter than they are directed by the theory.

Afterwards, in the ſame ſummer, I made ſuch another expedition, in a ſailing veſſel, along with Captain Campbell, of the Mary yacht, and Dr. Knight. Having prepared two of theſe machines as near alike as poſſible, I determined to try, how far they were capable of agreement, when expoſed to the ſame inconveniencies, and uſed together. During the trial of theſe machines, one made 86,716 revolutions, and the other made 88,184. During this ſpace, they were compared at ten ſeveral intervals. The revolutions between each interval differed from the proportion of theſe numbers, in the firſt compariſon, one-nineteenth of the whole interval. The errors of each interval, in the other compariſons, were, in order, two-ſeventeenths, one-nineteenth, one-twentieth, one-fifty-fourth, one-fourteenth, one-eightieth, one-ſixty-ſeventh, one-fourteenth, one-ſixteenth; the greateſt errors being where the ſpaces were the ſhorteſt. In other reſpects, the plates ſeemed to perform their duty, in the water, well enough, though the ſea was as rough, in this voyage, as our ſmall veſſel would well bear.

Laſtly, being for ſome time on board the Fortune ſloop of war, commanded by Alexander Campbell, Eſquire; in company with Dr. Knight, for the purpoſe of making trial of his new invented ſea-compaſſes, I had frequent opportunities of making uſe of theſe machines, by comparing them with one another, with the log, and with real diſtances; and having, by repeated trials, pretty well aſcertained the number of turns of the plate, that was equal to a given ſpace, by the help of the log, in the manner before deſcribed, when the ſhip was upon a middle velocity; I found the ſpaces, ſo meaſured, nearly conſiſtent with themſelves, and with the truth: but all this while the winds and weather were very moderate. It afterwards happened, that we run eighteen leagues in a brisk gale of wind, which, though not fair for us (being

before

before the beam,) yet drove us ſometimes at the rate of eight knots an hour, as appeared by heaving the log. During this run I obſerved, that the reſiſtance of the water, to the line and plate, was very conſiderable, and increaſed the friction of the ſpindle ſo much, as to prevent it from beginning to turn, till the plate had twiſted the line to ſuch a degree, that when it did ſet a going, it would frequently run 150 or 200 turns at once. I alſo obſerved, that the wind coming acroſs the courſe of the ſhip, blew the cord a good deal out of the direction of the ſpindle, and cauſed the line to rub againſt the ſafeguard hole, for the button to ſtop againſt, as above deſcribed; which undoubtedly occaſioned conſiderable friction in that place. But the moſt untoward circumſtance that I obſerved, was, that being in a rough, but ſhort chopping ſea, and ſailing obliquely acroſs the waves, the plate would frequently be drawn from one wave to another through the air, without touching the water; and, as it appeared, would jump from one wave to another, the unevenneſs of the ſurface, joined to the quickneſs of the motion, not permitting the plate to follow the depreſſion of the water. This evil I endeavoured to remedy, by placing upon the line, at a ſmall diſtance before the plate, ſome hollow bullets, ſuch as are made for nets, in order to keep the plate ſo low down in the water, as to be below the bottom of the waves. This, in part, I found they did; but they, at the ſame time, added ſo much reſiſtance, in their paſſing through the water, that the inconvenience was as great one way as the other.

Upon making up the account of this run, I found the number of rotations were leſs, by one full third than they ought to have been, compared with former obſervations; which afforded me a convincing proof, that this inſtrument was conſiderably retarded in quick motions.

The length of the line made uſe of was about twenty fathoms, which I found neceſſary, that the water, diſturbed by the body of the ſhip, might be tolerably ſettled before the plate was drawn through it; but this length of line was alſo an inconvenience, as it met with greater reſiſtance in the water.

Upon the whole, it ſeems to me, that an inſtrument, made as above deſcribed, is capable of meaſuring the way of a ſhip at ſea, when its velocity does not exceed five ſea miles an hour, to a degree of exactneſs exceeding the log. It therefore may be uſeful in the menſuration of the velocities of tides, currents, &c. and alſo in meaſuring diſtances at ſea in taking ſurveys of coaſts, harbours, &c. Thus far it ſeems capable of performing, upon the ſuppoſition, that it cannot be brought to a greater degree of perfection. But this I am very far from ſuppoſing; on the contrary, I do not

not deſpair, that it may be brought to anſwer the end of meaſuring the way of a ſhip at ſea univerſally; and, for that reaſon, it may not be amiſs to put down a few hints, concerning the cauſe and cure of the errors above-mentioned, for the ſake of thoſe, who may hereafter be inclined to proſecute theſe inquiries *.

It appears then from the preceding obſervations, that the rotation of the plate is conſiderably retarded in the quickeſt motions of the ſhip; and ſenſibly ſo, in all velocities exceeding five miles an hour. This may proceed, firſt, from the friction of the machine increaſing in a greater proportion than the power to turn it round. Secondly, from the water's being put in motion by the ſhip, ſo as to follow it in the ſame direction, and that to a conſiderable diſtance aſtern. And, thirdly, from the plate's jumping from wave to wave, when their concavity is great and diſtance little.

The firſt may, in ſome meaſure, be helped, by applying a loaded fly, of a proper ſize, to the ſpindle of the machine, which will prevent its ſticking faſt for a time, and then whirling round with great rapidity, as it is apt to do when the reſiſtance is great; by which means, the motion will be rendered more equal and uniform, as was juſtly obſerved to me by my friend Mr. Ellicott, of this Society.

Alſo, if the body of the machine were hung, equally poiſed upon croſs-centres, like thoſe uſed for ſea-compaſſes, or in the manner of a ſwivel-gun, as Captain Alexander Campbell well propoſed; the ſpindle of the machine would readily place itſelf in the ſame direction with the line that draws it, and thereby avoid unneceſſary frictions from the oblique direction of the cord.

The ſecond may be helped by placing the machine upon the end of a pole, faſtened near the forecaſtle, over the ſide of the ſhip. By this means, a ſhorter line will be neceſſary, and the plate prevented from working in the more diſturbed water at the ſtern.

Laſtly, its quitting the water, perhaps, might be helped by joining a ſhank of braſs, of ſix inches long, and three quarters of an inch diameter, to the fore-part of

* Upon communicating theſe experiments and obſervations to my ingenious friend Mr. William Ruſſel, he gave me an account of a machine, that he had made trial of in a voyage, ſome years ſince, from the Levant, ſo nearly agreeing with the above deſcribed, that one would have imagined we had been of each other's council in deſigning them.

the plate, to which the cord muſt be faſtened, the ends of the ſhank being formed into a figure moſt convenient for paſſing through the water with eaſe. The weight of this will cauſe the fore-part of the plate to ſink faſter than the other, and endeavour to give it a direction down into the water *.

I had intended to have made trial of the effect of theſe alterations, but have been prevented, partly by want of opportunity, and partly from the indifference, with which I found ſuch a contrivance as this, even if brought to perfection, was likely to be received by ſeamen; who, in general, do not ſeem to be over fond of making trial of new inſtruments, eſpecially if propoſed by landmen, as, in deriſion, they are pleaſed to call us.

Indeed it may be objected, that, could we meaſure the way of a ſhip through the water ever ſo exactly, unleſs ſome method were found out, of aſcertaining the currents, &c.; a ſhip's courſe, with reſpect to the globe, could not hereby be determined. But then it may be replied, with equal juſtice, that it is for want of a means of meaſuring the way of a ſhip through the water (and this compared with other check obſervations,) that the drift and velocities of the principal currents have not already been determined.

Mr. de Saumarez, in his ſecond paper, of March 1729, makes mention of another machine for this purpoſe, which he himſelf acknowledges to be inferior to his former, eſpecially in rough weather at ſea. But as ſeveral others have fallen upon, and propoſed, a machine ſimilar to this; it may not be amiſs to add the following remarks upon it. The firſt mover, in this, is compoſed of four arms, fixed to the bottom of a perpendicular ſpindle; each arm is furniſhed with a vane, which opens one way, and ſhuts the other, as ſome have attempted the making of horizontal wind-mills. This, by being carried through the water progreſſively, will turn round, and the faſter, as the ſhip moves faſter: but to judge, whether it will do it proportionably in all velocities of the ſhip, let us conſider,

1. That a good ſailing ſhip will frequently ſail at the rate of ten ſea miles (ſixty to a degree) an hour, which is at the rate of ſeventeen feet *per* ſecond.

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* Mr. Ruſſel's plate was joined to a ſhank, who never found it to jump out of the water, at any time, when he made uſe of it.

2. Suppoſing

2. Suppoſing the ſide of the fly, where the vanes are cloſed, to be retained by the water at reſt; the oppoſite ſide of the fly, where the vane is open, muſt meet the water with a velocity double to that of the ſhip, or at the rate of thirty-four feet in a ſecond: as would be the caſe with the upper part of a coach-wheel, whoſe velocity through the air is double to that, wherewith the coach moves forward.

3. That a plane ſurface of three inches ſquare, moving through the water with a velocity of thirty-four feet *per* ſecond, will meet with a reſiſtance, at leaſt, equal to ſeventy pounds avoirdupoiſe.

4. That the reſiſtance, which the open vanes will meet with in the water, will, in ſwift motions, be very conſiderable, and, of conſequence, the fly will move much ſlower than it ought to do, if theſe reſiſtances were leſs.

5. That from hence there is much reaſon to doubt, whether the reſiſtance of the medium, and friction of the machine, taken together, will always produce ſuch diminution, in the number of turns, as that the number of revolutions, actually ſhewn by the indexes, may be the ſame when the ſame ſpace is gone over with a great velocity, as with a ſmall one.

AN

AN ACCOUNT of some Improvements of the Mariner's Compass, in order to render the Card and Needle proposed by Dr. KNIGHT, of general Use, by JOHN SMEATON, Philosophical Instrument Maker.

Presented July 5th, 1750, and printed, with Alterations.

THE cover of the wooden box being taken off, the compass is in a condition to be made use of in the binacle, when the weather is moderate: but if the sea runs high, as the inner box is hung very free on its centres, (the better to answer its other purposes,) it will be necessary to slacken the milled nut, placed upon one of the axes that supports the ring, and to tighten the nut on the inside that corresponds to it. By this means the inner box and ring will be lifted up from the edges, upon which they rest when free; and the friction will be increased, and that to any degree necessary to prevent the too great vibration, which otherwise would be occasioned by the motion of the ship.

To make the compass useful in taking the magnetic azimuth, or amplitude of the sun and stars, as also the bearing of headlands, ships, and other objects, at a distance, the brass edges, designed at first to support the card, and throw the weight thereof as near the circumference as possible, is itself divided into degrees and halves; which may be easily estimated into smaller parts, if necessary.

The divisions are determined by means of a cat-gut line, stretched perpendicularly with the box, as near the brass edge as may be, that the parallax arising from a different position of the observer may be as little as possible.

Underneath the card are two small weights, sliding on two wires, placed at right angles to each other, which being moved nearer to, or farther from the centre, counterbalance the dipping of the card in different latitudes, or restore the *equilibrium* of it, where it happens by any other means to be got too much out of level.

There is also added an *index* at the top of the inner box, which may be put on and taken off at pleasure, and serves for all altitudes of the object. It consists of a bar

equal

equal in length to the diameter of the inner box, each end being furniſhed with a perpendicular ſtile, with a ſlit parallel to the ſides thereof. One of the ſlits is narrow, to which the eye is applied, and the other is wider, with a ſmall cat-gut ſtretched up the middle of it, and from thence continued horizontally from the top of one ſtile to the top of the other: there is alſo a line drawn along the upper ſurface of the box. Theſe four, viz. the narrow ſlit, the horizontal cat-gut thread, the perpendicular one, and the line on the bar, are in the ſame plane, which diſpoſes itſelf perpendicular to the horizon, when the inner box is at reſt, and hangs free. This index does not move round, but is always placed on ſo as to anſwer the ſame ſide of the box.

When the ſun's azimuth is deſired, and his rays are ſtrong enough to caſt a ſhadow, turn about the wooden box, till the ſhadow of the horizontal thread, or (if the ſun be too low) till that of the perpendicular thread in one ſtile, or the light through the ſlit in the other, falls upon the line on the index bar, or vibrates to an equal diſtance on each ſide of it, gently touching the box, if it vibrates too far: obſerve at the ſame time the degree marked upon the braſs edge by the cat-gut line. In counting the degree for the azimuth, or any other angle that is reckoned from the meridian, make uſe of the outward circle of figures upon the braſs edge, and the ſituation of the index bar, with regard to the card and needle, will always direct upon what quarter of the compaſs the object is placed.

But if the ſun does not throw out ſufficiently ſtrong, place the eye behind the narrow ſlit in one of the ſtiles, and turn the wooden box about, till ſome part of the horizontal or perpendicular thread appears to interſect the centre of the ſun, or vibrate to an equal diſtance on each ſide of it, uſing ſmoked glaſs next the eye, if the ſun's light is too ſtrong. In this method another obſerver will be generally neceſſary to note the degree out by the *nonius*, at the ſame time the firſt gives notice that the thread appears to ſplit the object.

From what has been ſaid, the other obſervations will be eaſily performed; only in caſe of the ſun's amplitude, take care to number the degrees by the half of the inner circle of figures on the card, which are the complements of the outer to ninety, and conſequently ſhew the diſtance from eaſt or weſt.

The azimuth of the ſtars may alſo be obſerved by night: a proper light ſerving equally for one obſerver to ſee the thread, and the other the degree upon the card.

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MARINERS COMPASS.

Fig. 1.

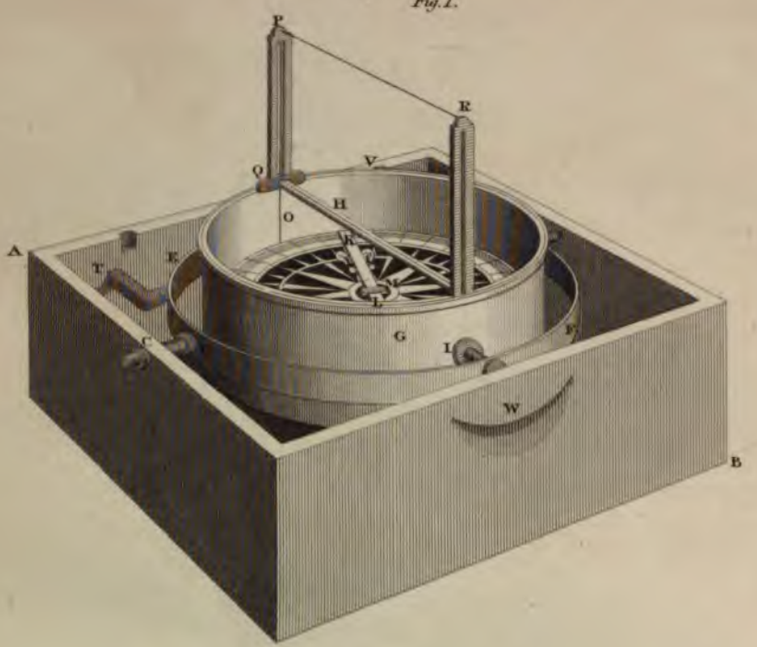
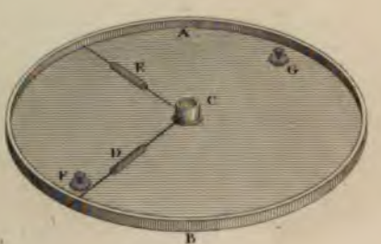


Fig. 4.

Fig. 2.

Fig. 3.



It may not be amiſs to remark farther, that, in caſe the inner box ſhould loſe its *equilibrium*, and conſequently the index be out of the plane of a vertical circle, an accurate obſervation may ſtill be made, provided the ſun's ſhadow is diſtinct: for, by obſerving firſt with one end of the index towards the ſun, and then the other, a mean of the two obſervations will be the truth.

Explanation of the figures, PLATE III.

Fig. 1, is a perſpective view of the compaſs, when in order for obſervation. The point of view being the centre of the card, and the diſtance of the eye two feet.

A B is the wooden box.

C and D are two milled nuts; by means whereof the axis of the inner box and ring are taken from the edges on which they move, and the friction increaſed when neceſſary.

E F is the ring that ſupports the inner box.

G H is the inner box; and

I is one of its axis, by which it is ſuſpended on the ring E F.

K L is the magnet, or needle; and

M a ſmall brace of ivory, that confines the cap to its place. See fig. 2.

The card is a ſingle varniſhed paper, reaching as far as the outer circle of figures, which is a circle of thin braſs, the edge whereof is turned down at right angles to the plane of the card, to make it more ſtiff.

O is a catgut line drawn down the inſide of the box; for determining the degree upon the braſs edge.

P Q R S is the index bar, with its two ſtiles and catgut threads; which, being taken off from the top of the box, is placed in two pieces T and V, notched properly to receive it.

W is a piece cut out in the wood, ſerving as a handle.

Fig. 2, is the card *in plano*, with the needle fixed upon it; being one third of the diameter of the reel card.

Fig. 3, is a perſpective view of the back ſide of the card, where

A B repreſents the turning down of the braſs edge.

C is the under part of the ivory cap.

D and E are the two ſliding weights to balance the card; and

F and G, two ſcrews that fix the braſs edge, &c. to the needle.

Fig. 4, is the pedeſtal that ſupports the card, containing a ſewing needle, fixed in two ſmall grooves to receive it, by means of the collet C, in the manner of a port crayon.

A D, the ſtem, is filed into an octagon, that it may be the more eaſily unſcrewed.

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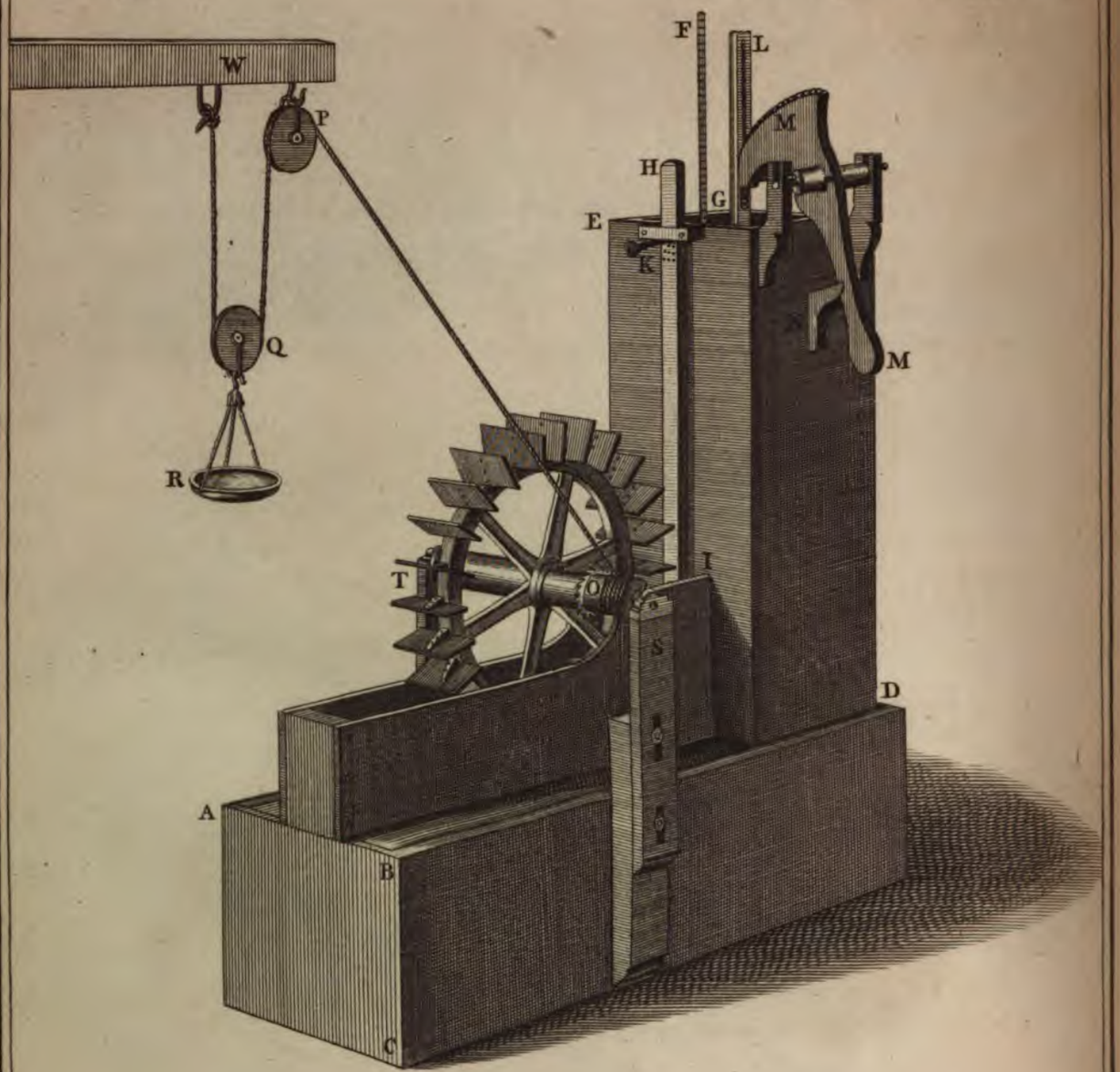
Plate 4. page 27. *Philos. Trans. Vol. LI.* TAB. IV. *p. 101.*



J. Smeaton delin. *J. Mynde Sc.*

AN Experimental Enquiry concerning the Natural Powers of Water and Wind to turn Mills, and other Machines, depending on a Circular Motion.

Read before the Royal Society, May 3d and 10th, 1759.

WHAT I have to communicate on this ſubject was originally deduced from experiments made on working models, which I look upon as the beſt means of obtaining the outlines in mechanical enquiries. But in this caſe it is very neceſſary to diſtinguiſh the circumſtances in which a model differs from a machine in large; otherwiſe a model is more apt to lead us from the truth than towards it. Hence the common obſervation, that a thing may do very well in a model that will not anſwer in large. And, indeed, though the utmoſt circumſpection be uſed in this way, the beſt ſtructure of machines cannot be fully aſcertained, but by making trials with them, when made of their proper ſize. It is for this reaſon that though the models referred to, and the greateſt part of the following experiments, were made in the years 1752 and 1753, yet I deferred offering them to the Society, until I had an opportunity of putting the deductions made therefrom in real practice, in a variety of caſes, and for various purpoſes; ſo as to be able to aſſure the Society that I have found them to anſwer.

PART I.

Concerning Undershot Water Wheels,

PLATE IV. Fig. 1, is a perſpective view of the machine for experiments on water wheels; wherein

A B C D is the lower ciſtern, or magazine, for receiving the water, after it has quitted the wheel; and for ſupplying

D E the upper ciſtern, or head; wherein the water being raiſed to any height required, by a pump, that height is ſhewn by

F G, a ſmall rod, divided into inches and parts; with a float at the bottom, to move the rod up and down, as the ſurface of the water riſes and falls.

H I is a rod by which the ſluice is drawn, and ſtopped at any height required, by means of

K a pin, or peg, which fits ſeveral holes, placed in the manner of a diagonal ſcale, upon the face of the rod H I.

G L is the upper part of the rod of the pump, for drawing the water out of the lower ciſtern, in order to raiſe and keep up the ſurface thereof at its deſired height, in the head D E; thereby to ſupply the water expended by the aperture of the ſluice.

M M is the arch and handle for working the pump, which is limited in its ſtroke by

N, a piece for ſtopping the handle from raiſing the piſton too high; that alſo being prevented from going too low, by meeting the bottom of the barrel.

O is the cylinder, upon which a cord winds, and which, being conducted over the pullies P and Q, raiſes

R, the ſcale, into which the weights are put, for trying the power of the water.

S T, the two ſtandards, which ſupport the wheel, are made to ſlide up and down, in order to adjuſt the wheel, as near as poſſible to the floor of the conduit.

W the beam which ſupports the ſcale and pullies; this is repreſented as but little higher than the machine, for the ſake of bringing the figure into a moderate compaſs, but in reality is placed fifteen or ſixteen feet higher than the wheel.

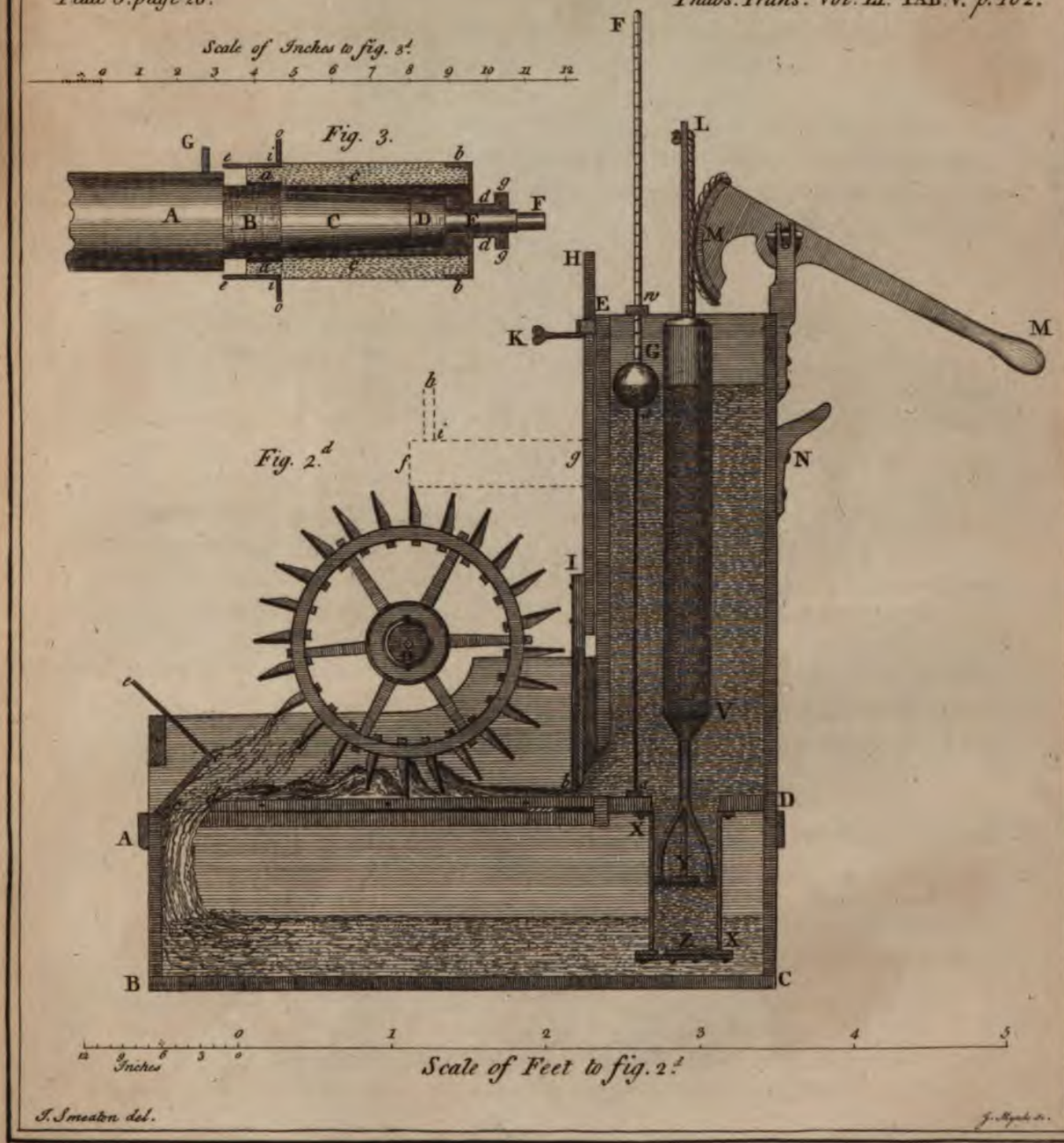
PLATE V. Fig. 2, is a ſection of the ſame machine, wherein the ſame parts are marked with the ſame letters as in fig. 1. Beſides which

X X is the pump barrel, being five inches diameter, and eleven inches long.

Y is the piſton; and

Z the fixed valve.

G V is a cylinder of wood, fixed upon the pump-rod, and reaches above the ſurface of the water: this piece of wood being of ſuch a thickneſs, that its ſection is half the area


Scale of Inches to fig. 3d.
Fig. 3.
G
A
B
C
D
E
F
H
K
L
M
N
I
V
X
Y
Z
Fig. 2d
Scale of Feet to fig. 2d
Inches
T. Smeaton del.

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area of that of the pump-barrel, will cauſe the ſurface of the water to riſe in the head, as much while the piſton is deſcending, as while it is riſing: and will thereby keep the gauge-rod F G more equally to its height.—*Note*, The arch and handle M M is here repreſented on a different ſide to what it is ſhewn in the preceding figures, in order that its dimenſions may the better appear.

a a ſhews one of the two wires which ſerve as directors to the float, in order that the gauge rod F G may be kept perpendicular; for the ſame purpoſe alſo ſerves *w*, a piece of wood with a hole to receive the gauge-rod, and keep it upright.

b is the aperture of the ſluice.

c c a kant-board, for throwing the water more directly down the opening *c d*, into the lower ciſtern; and

c e is a ſloping board, for bringing back the water that is thrown up by the floats of the wheel.

Fig. 3, repreſents one end of the main axis, with a ſection of the moveable cylinder, marked O in the preceding figures.

A B C D is the end of the axis; whereof the parts

B and D are covered with ferrules or hoops of braſs.

E is a cylinder of metal; whereof the part marked

F is the pivot or gudgeon.

c c is the ſection of an hollow cylinder of wood, the diameter of the interior part being ſomewhat larger than the cylindrical ferrule B.

a a is the ſection of a ferrule of braſs, driven into the end of the hollow cylinder, and which is adjuſted to that marked B, ſo as to ſlide freely thereupon, but with as little ſhake as poſſible.

b b, *d d*, *g g*, repreſent the ſection of a braſs ferrule, plate, and ſocket, fixed upon the other end of the hollow cylinder; the ſocket *d d* being adjuſted to ſlide freely upon

upon the cylinder E, in the ſame manner as the ferrule *a a* ſlides upon the cylinder B: the outer of the ſocket at

g g is formed into a ſort of button; by puſhing whereof, the hollow cylinder will move backwards and forwards, or turn round at pleaſure upon the cylindrical parts of the axis B and E.

e e, *i i*, *o o*, repreſent the ſection of a braſs ferrule, alſo fixed upon the hollow cylinder: the edge of this ferrule

e e is cut into teeth, in the manner of a *contrate* wheel; and the edge thereof

o o is cut in the manner of a ratchet.

Of conſequence, when the plate *b d d b* is puſhed cloſe to the ferrule D, the teeth of the ferrule *e e* will lay hold of

G, a pin fixed into the axis; by which means the hollow cylinder is made to turn along with the wheel and axis: but being drawn back by the button *g g*, the hollow cylinder is thereby diſengaged from the pin G, and ceaſes turning.—*Note*, The weight in the ſcale is prevented from running back, by a catch that plays in and lays hold of the ratchet *o o*.

By this means the hollow cylinder, upon which the cord winds and raiſes the weight, is put in action and diſcharged therefrom inſtantaneouſly, while the wheel is in motion: for, without ſome contrivance of this kind, it would not be eaſy to make this ſort of experiments with any tolerable degree of exactneſs.

The uſe of the apparatus now deſcribed will be rendered more intelligible, by giving a general idea of what I had in view; but as I ſhall be obliged to make uſe of a term which has heretofore been the cauſe of diſputation, I think it neceſſary to aſſign the ſenſe in which I would be underſtood to uſe it; and in which I apprehend it is uſed by practical *mechanics*.

The word *power*, as uſed in practical mechanics, I apprehend, to ſignify the exertion of ſtrength, gravitation, impulſe, or preſſure, ſo as to produce motion: and by means of ſtrength, gravitation, impulſe, or preſſure, compounded with motion, to be capable of

of producing an effect: and that no effect is properly mechanical, but what requires ſuch a kind of power to produce it.

The raiſing of a weight, relative to the height to which it can be raiſed in a given time, is the moſt proper meaſure of power; or, in other words, if the weight raiſed is multiplied by the height to which it can be raiſed in a given time, the product is the meaſure of the power raiſing it; and conſequently, all thoſe powers are equal, whoſe products, made by ſuch multiplication, are equal: for if a power can raiſe twice the weight to the ſame height; or the ſame weight to twice the height, in the ſame time that another power can, the firſt power is double the ſecond: and if a power can raiſe half the weight to double the height; or double the weight to half the height; in the ſame time that another can, thoſe two powers are equal. But *note*, all this is to be underſtood in caſe of ſlow or equable motion of the body raiſed; for in quick, accelerated, or retarded motions, the *vis inertiæ* of the matter moved will make a variation.

In comparing the effects produced by water-wheels with the powers producing them; or, in other words, to know what part of the original power is neceſſarily loſt in the application, we muſt previouſly know how much of the power is ſpent in overcoming the friction of the machinery, and the reſiſtance of the air; alſo what is the real velocity of the water at the inſtant that it ſtrikes the wheel; and the real quantity of water expended in a given time.

From the velocity of the water, at the inſtant that it ſtrikes the wheel, given, the height of head productive of ſuch velocity can be deduced, from acknowledged and experimented principles of hydroſtatics: ſo that by multiplying the quantity, or weight of water, really expended in a given time, by the height of a head ſo obtained; which muſt be conſidered as the height from which that weight of water had deſcended in that given time; we ſhall have a product, equal to the original power of the water, and clear of all uncertainty that would ariſe from the friction of the water, in paſſing ſmall apertures; and from all doubts, ariſing from the different meaſure of ſpouting waters, aſſigned by different authors. On the other hand, the ſum of the weights raiſed by the action of this water, and of the weight required to overcome the friction and reſiſtance of the machine, multiplied by the height to which the weight can be raiſed in the time given, the product will be equal to the effect of that power; and the proportion of the two products will be the proportion of the *power* to the *effect*: ſo that by loading the wheel with different weights ſucceſſively, we ſhall be able to determine at what particular load, and velocity of the wheel, the effect is a *maximum*.

The

The manner of finding the real velocity of the water, at the inſtant of its ſtriking the wheel; the manner of finding the value of the friction, reſiſtance, &c. in any given caſe; and the manner of finding the real expence of water, ſo far as concerns the following experiments, without having recourſe to theory; being matters upon which the following determinations depend, it will be neceſſary to explain them.

To determine the Velocity of the Water ſtriking the Wheel.

It has already been mentioned, in the references to the figures, that weights are raiſed by a cord winding round a cylindrical part of the axis. Firſt, then, let the wheel be put in motion by the water, but without any weights in the ſcale; and let the number of turns in a minute be ſixty: now it is evident, that were the wheel free from friction and reſiſtance, that ſixty times the circumference of the wheel would be the ſpace through which the water would have moved in a minute; with that velocity wherewith it ſtruck the wheel: but the wheel being incumbered by friction and reſiſtance, and yet moving ſixty turns in a minute, it is plain that the velocity of the water muſt have been greater than ſixty circumferences before it met with the wheel. Let now the cord be wound round the cylinder, but contrary to the uſual way, and put a weight in the ſcale; the weight ſo diſpoſed (which may be called the *counter-weight*) will endeavour to aſſiſt the wheel in turning the ſame way, as it would have been turned by the water: put therefore as much weight into the ſcale as, without any water, will cauſe it to turn ſomewhat faſter than at the rate of ſixty turns in a minute; ſuppoſe ſixty-three; let it now be tried again by the water, aſſiſted by the weight; the wheel therefore will now make more than ſixty turns; ſuppoſe ſixty-four: hence we conclude the water ſtill exerts ſome power in giving motion to rhe wheel. Let the weight be again increaſed, ſo as to make ſixty-four and a half turns in a minute without water: let it once more be tried with water as before; and ſuppoſe it now to make the ſame number of turns with water as without, viz. ſixty-four and a half: hence it is evident, that in this caſe the wheel makes the ſame number of turns in a minute, as it would do if the wheel had no friction or reſiſtance at all; becauſe the weight is equivalent thereto; for were it too little the water would accelerate the wheel beyond the weight; and if too great, retard it; ſo that the water now becomes a *regulator* of the wheel's motion; and the velocity of its circumference becomes a meaſure of the velocity of the water.

In like manner, in ſeeking the greateſt product, or *maximum*, of effect; having found by trials what weight gives the greateſt product, by ſimply multiplying the weight

weight in the ſcale by the number of turns of the wheel, find what weight in the ſcale, when the cord is on the contrary ſide of the cylinder, will cauſe the wheel to make the ſame number of turns the ſame way, without water; it is evident that this weight will be nearly equal to all friction and reſiſtance taken together; and conſequently, that the weight *in* the ſcale, with twice * the weight *of* the ſcale, added to the back or counter-weight, will be equal to the weight that could have been raiſed, ſuppoſing the machine had been without friction or reſiſtance; and which multiplied by the height to which it was raiſed, the product will be the greateſt effect of that power.

The Quantity of Water expended is found thus:

The pump made uſe of for repleniſhing the head with water was ſo carefully made, that, no water eſcaping back by the leathers, it delivered the ſame quantity of water at every ſtroke, whether worked quick or ſlow; and as the length of the ſtroke was limited, conſequently the value of one ſtroke (or, on account of more exactneſs, twelve ſtrokes) was known, by the height to which the water was thereby raiſed in the head; which, being of a regular figure, was eaſily meaſured. The ſluice, by which the water was drawn upon the wheel, was made to ſtop at certain heights by a peg; ſo that when the peg was in the ſame hole, the aperture for the effluent water was the ſame. Hence the quantity of water expended by any given head, and opening of the ſluice, may be obtained: for, by obſerving how many ſtrokes a minute was ſufficient to keep up the ſurface of the water at the given height, and multiplying the number of ſtrokes by the value of each, the water expended by any given aperture and head in a given time will be given.

Theſe things will be further illuſtrated by going over the *calculus* of one ſet of experiments.

Specimen of a Set of Experiments.

The ſluice drawn to the firſt hole.

The water above the floor of the ſluice - - - - - -	30 inches.
Strokes of the pump in a minute - - - - - - -	39½
The head raiſed by 12 ſtrokes - - - - - - - -	21
The wheel raiſed the empty ſcale, and made turns in a minute - -	80
With a counter-weight of 1 lb. 8 oz. it made - - - - - - - -	85
Ditto tried with water - - - - - - - - - - - - - -	86

* The weight of the ſcale makes part of the weight both ways.

No.	Weight. lb. oz.	Turns in a minute.	Product.
1	4 0	45	180
2	5 0	42	210
3	6 0	36½	217½
4	7 0	33⅔	236½
5	8 0	30	240 maximum.
6	9 0	26½	238½
7	10 0	22	220
8	11 0	16½	181½
9	12	* ceaſed working.	

Counter-weight, for 30 turns without water, 2 oz. in the ſcale.

N. B. The area of the head was 105,8 ſquare inches.
Weight of the empty ſcale and pulley, 10 oz.
Circumference of the cylinder, 9 inches.
Circumference of the water-wheel, 75 ditto.

Reduction of the above Set of Experiments.

The circumference of the wheel, 75 inches, multiplied by 86 turns, gives 6450 inches for the velocity of the water in a minute; $\frac{1}{60}$ of which will be the velocity in a ſecond, equal to 107,5 inches, or 8,96 feet, which is due to a head of fifteen inches †; and this we call the *virtual* or *effective* head.

The area of the head being 105,8 inches, this multiplied by the weight of water of the inch cubic, equal to the decimal ,579 of the ounce avoirdupoiſe, gives 61,26 ounces for the weight of as much water, as is contained in the head, upon one inch in depth, $\frac{1}{16}$ of which is 3,83 pounds; this multiplied by the depth 21 inches, gives 80,43lb. for the value of 12 ſtrokes; and by proportion, 39½ (the number made in a minute) will give 264,7 lb. the weight of water expended in a minute.

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* *N. B.* When the wheel moves ſo ſlow as not to rid the water ſo faſt as ſupplied by the ſluice, the accumulated water falls back upon the aperture, and the wheel immediately ceaſes moving.

† This is determined upon the common maxim of hydroſtatics, that the velocity of ſpouting waters is equal to the velocity that an heavy body would acquire in falling from the height of the reſervoir; and is proved by the riſing of jets to the height of their reſervoirs nearly.

Now,

Now, as 264,7 lb. of water may be conſidered as having deſcended through a ſpace of 15 inches in a minute, the product of theſe two numbers 3970 will expreſs the *power* of the water to produce mechanical effects; which were as follows:

The velocity of the wheel at the *maximum*, as appears above, was 30 turns a minute; which multiplied by 9 inches, the circumference of the cylinder, makes 270 inches; but as the ſcale was hung by a pulley and double line, the weight was only raiſed half of this, viz. 135 inches.

The weight in the ſcale at the *maximum*	8 *lb.*	0 *oz.*	
Weight of the ſcale and pulley - -	0	10	
Counter-weight, ſcale, and pulley - -	0	12	
Sum of the reſiſtance - - -	9	6	
or *lb.*	9,375.		

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Now, as 9,375 lb. is raiſed 135 inches, theſe two numbers being multiplied together, the product is 1266, which expreſſes the effect produced at a *maximum*: ſo that the proportion of the *power* to the *effect* is as 3970: 1266, or as 10: 3,18.

But though this is the greateſt *ſingle* effect producible from the power mentioned, by the impulſe of the water upon an underſhot wheel; yet, as the whole power of the water is not exhauſted thereby, this will not be the true ratio between the *power* of the water, and the *ſum* of all the *effects* producible therefrom: for as the water muſt neceſſarily leave the wheel with a velocity equal to the wheel's circumference, it is plain that ſome part of the power of the water muſt remain after the quitting the wheel.

The velocity of the wheel at the *maximum* is 30 turns a minute; and conſequently its circumference moves at the rate of 3,123 feet a ſecond, which anſwers to a head 1,82 inches; this being multiplied by the expence of water in a minute, viz. 264,7 lb. produces 481 for the power *remaining* in the water after it has paſſed the wheel: this being therefore deducted from the original power 3970, leaves 3489, which is that *part* of the power which is ſpent in producing the effect 1266; and conſequently the part of the power ſpent in producing the effect, is to the greateſt effect producible thereby as 3489: 1266: : 10: 3,62, or as 11 to 4.

The *velocity of the water* ſtriking the wheel has been determined to be equal to 86 circumferences of the wheel *per* minute, and the *velocity of the wheel* at the *maximum* to be 30; the velocity of the water will therefore be to that of the wheel as 86 to 30; or as 10 to 3,5, or as 20 to 7.

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The *load at the maximum* has been ſhown to be equal to 9 lb. 6 oz. and that the wheel ceaſed moving with 12 lb. in the ſcale: to which, if the weight of the ſcale is added, viz. 10 ounces *, the proportion will be nearly as 3 to 4 between the load at the *maximum* and *that* by which the wheel is ſtopped.

It is ſomewhat remarkable, that though the velocity of the wheel in relation to the water turns out greater than $\frac{1}{3}$ of the velocity of the water, yet the impulſe of the water in the caſe of a *maximum* is more than double of what is aſſigned by theory; that is, inſtead of $\frac{4}{9}$ of the column, it is nearly equal to the whole column.

It muſt be remembered, therefore, that in the preſent caſe, the wheel was not placed in an open river, where the natural current, after it has communicated its impulſe to the float, has room on all ſides to eſcape, as the theory ſuppoſes; but in a conduit, or race, to which the float being adapted, the water cannot otherwiſe eſcape than by moving along with the wheel. It is obſervable, that a wheel working in this manner, as ſoon as the water meets the float, receiving a ſudden check, it riſes up againſt the float, like a wave againſt a fixed object; inſomuch that when the ſheet of water is not a quarter of an inch thick before it meets the float, yet this ſheet will act upon the whole ſurface of a float, whoſe height is three inches; and conſequently were the float no higher than the thickneſs of the ſheet of water, as the theory alſo ſuppoſes, a great part of the force would have been loſt, by the water daſhing over the float †.

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* The reſiſtance of the air in this caſe ceaſes, and the friction is not added, as 12 lb. in the ſcale was ſufficient to ſtop the wheel after it had been in full motion; and therefore somewhat more than a counterbalance to the impulſe of the water.

† Since the above was written, I find that Profeſſor Euler, in the Berlin Acts for the year 1748, in a memoir entitled *Maximis pour arranger le plus avantageusment les machines destinées à elever de l'eau par le moyen de pompes*, page 192. § 9. has the following paſſage; which ſeems to be the more remarkable, as I do not find he has given any demonſtration of the principle therein contained, either from theory or experiment; or has made any uſe thereof in his calculations on this ſubject:—"Cependant dans ce cas "puisque l'eau est reflechie, & qu'elle decoule sur les aubes vers les cotés, elle y exerce encore une force "particuliere, dont l'effet de l'impulsion sera augmenté; & experience jointe a la theorie a fait voir que "dans ce cas, la force est presque double: de sorte qu'il faut prendre le double de le section du fil d'eau "pour ce qui repond dans ce cas a le surface des aubes, pourvu qu'elles soient assez larges pour recevoir "ce supplement de force. Car si les aubes nétoient plus larges que le fil, on trait d'eau on ne devroit "prendre que ne simple section, tout comme dans le premier cas, ou l'aube toute entire est pappee "par l'eau."

TABLE

TABLE I.

Number.	Height of water in the cistern.	Turns of the wheel un loaded.	Virtual head deduced therefrom.	Turns at the maximum.	Load at the equilibrium.		Load at the maximum.		Water expended in a minute.	POWER.	EFFECT.	Ratio of the power and effect.	Ratio of the velocity of the water and wheel.	Ratio of the load at the equilibium, to the load at the maximum.	Experiments.
	In.		In.		lb.	oz	lb.	oz.							
1	33	88	15,85	30,	13	10	10	9	274,	4358	1411	10:3,24	10:3,4	10:7,75	
2	30	86	15.0	30,	12	10	9	6	264,7	3970	1266	10:3,2	10:3,5	10:7,4	
3	27	82	13,7	28,	11	2	8	6	243,	3329	1044	10:3,15	10:3,4	10:7,5	
4	24	78	12,3	27,7	9	10	7	5	235,	2890	901,4	10:3,12	10:3,55	10:7,53	At
5	21	75	11,4	25,9	8	10	6	5	214,	2439	735 7	10:3,02	10:3,45	10:7,32	the
6	18	70	9,95	23,5	6	10	5	5	199,	1970	561,8	10:2,85	10:3,36	10:8,02	1st
7	15	65	8,54	23,4	5	2	4	4	178,5	1524	442,5	10:2,9	10:3,6	10:8,3	hole.
8	12	60	7,29	22,	3	10	3	5	161,	1173	328	10:2,8	10:3,77	10:9,1	
9	9	52	5,47	19,	2	12	2	8	134,	733	213,7	10:2,9	10:3,65	10:9,1	
10	6	42	3,55	16,	1	12	1	10	114,	404,7	117	10:2,82	10:3,8	10:9,3	
11	24	84	14,2	30,75	13	10	10	14	342,	4890	1505	10:3,075	10:3,66	10:7,9	
12	21	81	13,5	29,	11	10	9	6	297,	4009	1223	10:3,01	10:3,62	10:8,05	
13	18	72	10,6	26,	9	10	8	7	285,	2993	975	10:3,25	10:3,6	10:8,75	At
14	15	69	9,6	25,	7	10	6	14	277,	2659	774	10:2,92	10:3,62	10:9,	the
15	12	63	8,0	25,	5	10	4	14	234,	1872	549	10:2,94	10:3,97	10:8,7	2d.
16	9	56	6,37	23,	4	0	3	13	201,	1280	390	10:3,05	10:4,1	10:9,5	
17	6	46	4,25	21,	2	8	2	4	167,5	712	212	10:2,98	10:4,55	10:9,	
18	15	72	10,5	29,	11	10	9	6	357,	3748	1210	10:3,23	10:4,02	10:8,05	
19	12	66	8,75	26,75	8	10	7	6	330,	2887	878	10:3,05	10:4,05	10:8,1	The
20	9	58	6,8	24,5	5	8	5	0	255,	1734	541	10:3,01	10:4,22	10:9,1	3d.
21	6	48	4,7	23,5	3	2	3	0	228,	1064	317	10:2,99	10:4,9	10:9,6	
22	12	68	9,3	27,	9	2	8	6	359,	3338	1006	10:3,02	10:3,97	10:9,17	
23	9	58	6,8	26,25	6	2	5	13	332,	2257	686	10:3,04	10:4,52	10:9,5	4th.
24	6	48	4,7	24,5	3	12	3	8	262,	1231	385	10:3,13	10:5,1	10:9,35	
25	9	60	7,29	27,3	6	12	6	6	355,	2588	783	10:3,03	10:4,55	10:9,45	5th.
26	6	50	5,03	24,6	4	6	4	1	307,	1544	450	10:2,92	10:4,9	10:9,3	
27	6	50	5,03	26,	4	15	4	9	360,	1811	534	10:2,95	10:5,2	10:9,25	6th.
1.	2.	3.	4.	5.	6.		7.		8.	9.	10.	11.	12.	13.	

In further confirmation of what is already delivered, I have adjoined a table, (TABLE I.) containing the result of 27 sets of experiments, made and reduced in the manner above specified. What remains of the theory of undershot wheels, will naturally follow from a comparison of the different experiments together.

Maxims and Observations deduced from the foregoing Table of Experiments.

Maxim I. That the virtual or effective head being the same, the effect will be nearly as the quantity of water expended.

This will appear by comparing the contents of the columns 4, 8, and 10, in the foregoing sets of experiments; as for

Example 1st, taken from No. 8, and 25, viz.

No.	Virtual Head.	Water expended.	Effect.
8	7,29	161	328
25	7,29	355	785

Now the heads being equal, if the effects are proportioned to the water expended, we shall have by maxim 1st, 161 : 355 :: 328 : 723; but 723 falls short of 785, as it turns out in experiment, according to No. 25, by 62; the effect therefore of No. 25, compared with No. 8, is greater than according to the present maxim in the ratio of 14 to 13.

The foregoing example, with four similar ones, are seen at one view in the following Table.

Examples.	No. Tab. I.	Virtual Head.	Expence of Water	Effect.	Comparison.	Variation.	Proportional Variation.
		Inch	*lb.*				
1st	8	7,29	161	328	161 : 355 : : 328 : 723	62 +	14 : 13
	25	7,29	355	785			
2d	13	10,5	285	975	285 : 357 : : 975 : 1221	11—	121 : 122
	18	10,5	357	1210			
3d	22	6,8	255	541	255 : 332 : : 541 : 704	18—	38 : 39
	23	6,8	332	686			
4th	21	4,7	228	317	228 : 262 : : 317 : 364	21 +	18 : 17
	24	4,7	262	385			
5th	26	5,03	307	450	307 : 360 : : 450 : 531	3 +	178 : 177
	27	5,03	360	534			

Hence therefore, in comparing different experiments, as ſome fall ſhort, and others exceed the *maximum*, and all agree therewith, as near as can be expected, in an affair where ſo many different circumſtances are concerned, we may, according to the laws of reaſoning by induction, conclude the maxim true; viz. that the effects are nearly as the quantity of water expended.

Maxim II. That the expence of water being the ſame, the effect will be nearly as the height of the virtual or effective head.

This alſo will appear by comparing the contents of columns 4, 8, and 10, in any of the ſets of experiments.

Example 1ſt, of No. 2, and No. 24, viz.

No.	Virtual Head.	Expence.	Effect.
2	15	264,7	1266
24	4,7	262	385

Now, as the expences are not quite equal, we muſt proportion one of the effects accordingly: thus

by maxim 1ſt, 262 : 264,7 : : 385 : 389
and by max. 2d, 15 : 4,7 : : 1266 : 397

Difference - - - - 8

The effect therefore of No. 24, compared with No. 2, is leſs than according to the preſent maxim in the ratio of 49 : 50.

The foregoing, and two other ſimilar examples, are compriſed in the following Table:

Examples.	No. Table I.	Virtual Head.	Expence of Water.	Effect.	Compariſon.	Variation.	Proportional Variation.
1ſt	2	15	264,7	1266	Max. 1ſt, 262 : 264,7 : : 385 : 319	8 —	49 : 50
	24	4,7	262	385	Max. 2d, 15 : 4,7 : : 1266 : 397		
2d	1	15,85	275	1411	Max. 1ſt, 114 : 275 : : 117 : 282	34—	8 : 9
	10	3,55	114	117	Max. 2d, 15,85 : 3,55 : : 1411 : 316		
3d	11	14,2	342	1505	Max. 1ſt, 167,5 : 342 : : 212 : 433	17—	25 : 26
	17	4,25	1675	212	Max. 2d, 14,2 : 4,25 : : 1505 : 450		

Maxim III. That the quantity of water expended being the ſame, the effect is nearly as the ſquare of its velocity.

This will appear by comparing the contents of columns 3, 8, and 10, in any of the ſets of experiments; as for

Example 1ſt, of No. 2, with No. 24, viz.

No.	Turns in a minute.	Expence.	Effect.
2	86	264,7	1266.
24	48	262	385

The velocity being as the number of turns, we ſhall have,

by maxim 1ſt, $262 : 264{,}7 : 385 : 389$

and by maxim 3d. $\left\{\begin{matrix} 86^2 : 48^2 \\ 7396 : 2304 \end{matrix}\right\} : 1266 : 394$

Difference - - - - - 5

The effect therefore of No. 24, compared with No. 2, is leſs than by the preſent maxim in the ratio of 78 : 79.

The foregoing, and three other ſimilar examples, are compriſed in the following Table:

Examples.	No. Table I.	Turns in a Minute.	Expence of Water.	Effect.	Compariſon.	Variation.	Proportional Variation.
1ſt	2	86	264,7	1266	Max. 1ſt, $262 : 264{,}7 :: 385 : 389$	5 —	78 : 79
	24	48	262	385	Max. 3d, $\left\{\begin{matrix} 86^2 : 48^2 \\ 7396 : 2304 \end{matrix}\right\} :: 1266 : 394$		
2	1	88	275	1411	Max. 1ſt, $114 : 275 :: 117 : 282$	39—	7 : 8
	10	42	114	117	Max. 3d, $\left\{\begin{matrix} 88^2 : 42^2 \\ 7744 : 1764 \end{matrix}\right\} :: 1411 : 321$		
3d	11	84	342	1505	Max. 1ſt, $167{,}5 : 342 :: 212 : 433$	18—	24 : 25
	17	46	167,5	212	Max. 3d. $\left\{\begin{matrix} 84^2 : 46^2 \\ 7056 : 2116 \end{matrix}\right\} :: 1505 : 451$		
4th	18	72	357	1210	Max. 1ſt, $228 : 357 :: 317 : 496$	42—	12 : 13
	21	48	228	317	Max. 3d, $\left\{\begin{matrix} 72^2 : 48^2 \\ 5184 : 2301 \end{matrix}\right\} :: 1210 : 538$		

Maxim IV. The aperture being the ſame, the effect will be nearly as the cube of the velocity of the water.

This alſo will appear by comparing the contents of columns 3, 8, and 10; as for

Example 1ſt, of No. 1, and No. 10, viz.

No.	Turns.	Expence.	Effect.
1	88	275	1411
10	42	114	117

Lemma. It muſt here be obſerved, that if water paſſes out of an aperture, in the ſame ſection, but with different velocities, the expence will be proportional to the velocity; and therefore converſely, if the expence is not proportional to the velocity, the ſection of the water is not the ſame.

Now, comparing the water diſcharged with the turns of No. 1, and 10, we ſhall have 88 : 42 : : 275 : 131,2; but the water diſcharged by No. 10, is only 114 lb. therefore, though the ſluice was drawn to the ſame height in No. 10, as in No. 1, yet the ſection of the water paſſing out, was leſs in No. 10, than No. 1, in the proportion of 114 to 131,2; conſequently, had the effective aperture or ſection of the water been the ſame in No. 10, as in No. 1, ſo that 131,2 lb. of water had been diſcharged inſtead of 114, the effect would have been increaſed in the ſame proportion; that is

by the *Lemma*,	88 :	42	: :	275 :	131,2
by maxim 1ſt,	114 :	131,2	: :	117 :	134,5
and by maxim 4th.	$\left\{\begin{matrix}83_3 \\ 681472\end{matrix}\right.$:	$\left.\begin{matrix}423 \\ 74088\end{matrix}\right\}$	: :	1411 :	153,5
	Difference - - - -				19

The effect therefore of No. 10, compared with No. 1, is leſs than it ought to be by the preſent maxim in the ratio of 7 : 8.

The foregoing, and three other ſimilar examples, are contained in the following Table:

Examples.	No. Table I.	Turns in a Minute.	Expence of Water.	Effect.	Compariſon.	Variation.	Proportional Variation.
1ſt	1	88	275	1411	Lemma. 88 : 42 :: 275 : 131,2	19—	7 : 8
	10	42	114	117	Max. 1. 114 : 131,2 :: 117 : 134,5		
					Max. 4. 88^3 : 42^3 :: 1411 : 153,5		
2d	11	84	342	1505	Lemma. 84 : 46 :: 342 : 187,3	10—	23 : 24
	17	46	167,5	212	Max. 1. 167,5 : 187,3 :: 212 : 237		
					Max. 4. 84^3 : 46^3 :: 1505 : 247		
3d	18	72	357	1210	Lemma. 72 : 48 :: 357 : 238	24—	14 : 15
	21	48	228	317	Max. 1. 228 : 238 :: 317 : 331		
					Max. 4. 72^3 : 48^3 :: 1210 : 355		
4th	22	68	359	1006	Lemma. 68 : 48 :: 359 : 253,4	18 +	20 : 19
	24	48	262	385	Max. 1. 262 : 253,4 :: 385 : 372		
					Max. 4. 68^3 : 48^3 :: 1006 : 354		

Obſervations.

Obſerv. 1ſt. On comparing column 2d and 4th, Table I. it is evident that the *virtual head* bears no certain proportion to the *head of water*; but that when the aperture is greater, or the velocity of the water iſſuing therefrom leſs, they approach nearer to a coincidence; and conſequently in the large openings of mills and ſluices, where great quantities of water are diſcharged from moderate heads, the head of water, and virtual head determined from the velocity, will nearly agree, as experience confirms.

Obſerv. 2d. Upon comparing the ſeveral proportions between the *power* and *effect* in column 11th, the moſt general is that of 10 to 3; the extremes 10 to 3,2 and 10 to 2,8; but as it is obſervable, that where the quantity of water, or the velocity thereof; that is, where the power is greateſt, the 2d term of the ratio is greateſt alſo: we may therefore well allow the proportion ſubſiſting in large works, as 3 to 1.

Obſerv.

Observ. 3d. The proportions of *velocities* between the *water* and *wheel* in column 12, are contained in the limits of 3 to 1 and 2 to 1; but as the greater velocities approach the limit of 3 to 1, and the greater quantity of water approaches to that of 2 to 1, the beſt general proportion will be that of 5 to 2.

Observ. 4th. On comparing the numbers in column 13, it appears, that there is no certain ratio between the *load* that the wheel will carry at its *maximum*, and what will totally ſtop it; but that they are contained within the limits of 20 to 19, and of 20 to 15; but as the effect approaches neareſt to the ratio of 20 to 15, or of 4 to 3, when the power is greateſt, whether by increaſe of velocity, or quantity of water, this ſeems to be the moſt applicable to large works; but as the load that a wheel ought to have, in order to work to the beſt advantage, can be aſſigned, by knowing the effect it ought to produce, and the velocity it ought to have in producing it; the exact knowledge of the greateſt load it will bear, is of the leſs conſequence in practice.

It is to be noted, that in all the examples under the laſt three of the four preceding maxims, the effect of the leſſer power falls ſhort of its due proportion to the greater, when compared by its maxim; except the laſt example of maxim fourth: and hence, if the experiments are taken ſtrictly, we muſt infer, that the effects increaſe and diminiſh in an higher ratio than thoſe maxims ſuppoſe; but as the deviation is not very conſiderable, the greateſt being about one-eighth of the quantity in queſtion; and as it is not eaſy to make experiments of ſo compounded a nature with abſolute preciſion; we may rather ſuppoſe, that the leſſer power is attended with ſome friction, or works under ſome diſadvantage, which has not been duly accounted for; and therefore we may conclude, that theſe maxims will hold very nearly, when applied to works in large.

After the experiments above mentioned were tried, the wheel, which had originally twenty-four floats, was reduced to twelve; which cauſed a diminution in the effect, on account of a greater quantity of water eſcaping between the floats and the floor; but a circular ſweep being adapted thereto, of ſuch a length, that one float entered the curve before the preceding one quitted it, the effect came ſo near to the former, as not to give hopes of advancing it by increaſing the number of floats beyond twenty-four in this particular wheel.

PART

PART II.

Concerning Overshot Wheels.

Read before the Royal Society, May 24th, 1759.

IN the former part of this eſſay, we have conſidered the impulſe of a confined ſtream, acting on *Undershot Wheels.* We now proceed to examine the power and application of water, when acting by its *gravity* on *Overshot Wheels.*

In reaſoning without experiment, one might be led to imagine, that however different the mode of application is; yet, that whenever the ſame quantity of water deſcends through the ſame perpendicular ſpace, that the natural effective power would be equal; ſuppoſing the machinery free from friction, equally calculated to receive the full effect of the power, and to make the moſt of it: for if we ſuppoſe the height of a column of water to be 30 inches, and reſting upon a baſe or aperture of one inch ſquare, every cubic inch of water that departs therefrom will acquire the ſame velocity, or *momentum*, from the uniform preſſure of 30 cubic inches above it, that one cubic inch let fall from the top will acquire in falling down to the level of the aperture; viz. ſuch a velocity as, in a contrary direction, would carry it to the level from whence it fell;* one would therefore ſuppoſe, that a cubic inch of water, let fall through a ſpace of 30 inches, and there impinging upon another body, would be capable of producing an equal effect by colliſion, as if the ſame cubic inch had deſcended through the ſame ſpace with a ſlower motion, and produced its effects gradually: for in both caſes gravity acts upon an equal quantity of matter, through an equal ſpace; † and conſequently, that whatever was the ratio between the power and effect in underſhot wheels, the ſame would obtain in overſhot, and indeed in all others: yet, however concluſive this reaſoning may ſeem, it will appear, in the courſe of the following deductions, that the effect of the gravity of deſcending bodies is very different from

* This a conſequence of the riſing of jets to the height of their reſervoirs nearly.

† Gravity, it is true, acts a longer ſpace of time upon the body that deſcends ſlowly than upon that which falls quickly; but this cannot occaſion the difference in the effect: for an elaſtic body falling through the ſame ſpace in the ſame time, will, by colliſion upon another elaſtic body, rebound nearly to the height from which it fell: or, by communicating its motion, cauſe an equal one to aſcend to the ſame height.

the

the effect of the ſtroke of ſuch as are *non elastic*, though generated by an equal mechanical power.

The alterations in the machinery already deſcribed, to accommodate the ſame for experiments on overſhot wheels, where principally as follows:

PLATE V. Fig. 2. The ſluice I *b* being ſhut down, the rod H I was unſcrewed and taken off.

The underſhot water-wheel was taken off the axis, and inſtead thereof an overſhot wheel of the ſame diameter was put into its place. *Note*, This wheel was two inches in the ſhroud or depth of the bucket; the number of the buckets was 36.

The ſtandards S and T, PLATE IV. were raiſed half an inch, ſo that the bottom of the wheel might be clear of ſtagnant water.

A trunk, for bringing the water upon the wheel, was fixed according to the dotted lines *f g*, Fig. 2. The aperture was adjuſted by a ſhuttle *h i*, which alſo cloſed up the outer end of the trunk, when the water was to be ſtopped.

Fig. 3. The ratchet *o o*, not being of one piece of metal with the ferrule *e e*, *i i* (though ſo deſcribed before, to prevent unneceſſary diſtinctions) was with its catch turned the contrary ſide; conſequently the moveable barrel would do its office equally, notwithſtanding the water-wheel, when at work, moved the contrary way.

Specimen

Specimen of a Set of Experiments.

Head 6 inches.

14 ½ ſtrokes of the pump in a minute, 12 ditto = 80 lb. *

Weight of the ſcale (being wet) 10½ oz.

Counterweight for 20 turns, beſides the ſcale, 3 oz.

No.	Weight in the Scale.	Turns.	Product.	Obſervations.
1	0 *lb.*	60		Threw moſt part of the water out of the wheel.
2	1	56		
3	2	52		
4	3	49	147	Received the water more quietly.
5	4	47	188	
6	5	45	225	
7	6	42½	255	
8	7	41	287	
9	8	38½	308	
10	9	36½	328½	
11	10	35½	355	
12	11	32⅝	360½	
13	12	31¼	375	
14	13	28½	370½	
15	14	27½	385	
16	15	26	390	
17	16	24½	392	
18	17	22¾	386¾	
19	18	21¾	391½	
20	19	20¾	394¼	maximum.
21	20	19¾	395	
22	21	18¼	388¼	
23	22	18	396	Worked irregular.
24	23	Overſet by its load.		

* The ſmall difference, in the value of 12 ſtrokes of the pump, from the former experiments, was owing to a ſmall difference in the length of the ſtroke, occaſioned by the warping of the wood.

Reduction

Reduction of the preceding Specimen.

In theſe experiments the head being 6 inches, and the height of the wheel 24 inches, the whole deſcent will be 30 inches: the expence of water was $14\frac{1}{2}$ ſtrokes of the pump in a minute, whereof 12 contained 80 lb.; therefore the water expended in a minute was $96\frac{2}{3}$ lb. which, multiplied by 30 inches, gives the *power* = 2900.

If we take the 20th experiment for the *maximum*, we ſhall have $20\frac{3}{4}$ turns in a minute, each of which raiſed the weight of $4\frac{1}{2}$ inches, that is, 93,37 inches in a minute. The weight in the ſcale was 19 lb. the weight of the ſcale $10\frac{1}{2}$ oz.; the counterweight 3 oz. in the ſcale, which, with the weight of the ſcale $10\frac{1}{2}$ oz. makes in the whole $20\frac{1}{2}$ lb. which is the whole reſiſtance or load: this, multiplied by 93,37 inches, makes 1914 for the effect.

The *ratio*, therefore of the *power* and *effect* will be as 2900 : 1914, or as 10 : 6,6, or as 3 : 2 nearly.

But if we compute the power from the height of the wheel only, we ſhall have $96\frac{2}{3}$ lb. multiplied by 24 inches = 2320 for the *power*, and this will be to the *effect* as 2320 : 1914, or as 10 : 8,2, or as 5 : 4 nearly.

The reduction of this ſpecimen is ſet down in No. 9, of the following Table; and the reſt were deduced from a ſimilar ſet of experiments, reduced in the ſame manner.

TABLE

TABLE II.

Containing the Result of Sixteen Sets of Experiments on Overshot Wheels.

Number.	Whole descent.	Water expended in a minute.	Turns at the maximum in a min.	Weight raised at the maximum.	Power of the whole descent.	Power of the Wheel.	Effect.	Ratio of the whole power and effect.	Ratio of power of the wheel and effect.	Mean ratio.
	Inch.	*lb.*		*lb.*						
1	27	30	19	$6\frac{1}{2}$	810	720	556	10 : 6,9	10 : 7,7	10:8,1
2	27	$56\frac{2}{3}$	$16\frac{1}{4}$	$14\frac{1}{2}$	1530	1360	1060	10 : 6,9	10 : 7,8	
3	27	$56\frac{2}{3}$	$20\frac{3}{4}$	$12\frac{1}{2}$	1530	1360	1167	10 : 7,6	10 : 8,4	
4	27	$63\frac{1}{2}$	$20\frac{1}{2}$	$13\frac{1}{2}$	1710	1524	1245	10 : 7,3	10 : 8,2	
5	27	$76\frac{2}{3}$	$21\frac{1}{2}$	$15\frac{1}{2}$	2070	1840	1500	10 : 7,3	10 : 8,2	
6	$28\frac{1}{2}$	$73\frac{1}{3}$	$18\frac{3}{4}$	$17\frac{1}{2}$	2090	1764	1476	10 : 7,	10 : 8,4	10:8,2
7	$28\frac{1}{2}$	$96\frac{2}{3}$	$20\frac{1}{4}$	$20\frac{1}{2}$	2755	2320	1868	10 : 6,8	10 : 8,	
8	30	90	20	$19\frac{1}{2}$	2700	2160	1755	10 : 6,5	10 : 8,1	10:8,2
9	30	$96\frac{2}{3}$	$20\frac{3}{4}$	$20\frac{1}{2}$	2900	2320	1914	10 : 6,6	10 : 8,2	
10	30	$113\frac{1}{3}$	21	$23\frac{1}{2}$	3400	2720	2221	10 : 6,5	10 : 8,2	
11	33	$56\frac{2}{3}$	$20\frac{1}{4}$	$13\frac{1}{2}$	1870	1360	1230	10 : 6,6	10 : 9,	10:8,5
12	33	$106\frac{2}{3}$	$22\frac{1}{4}$	$21\frac{1}{2}$	3520	2560	2153	10 : 6,1	10 : 8,4	
13	33	$146\frac{2}{3}$	23	$27\frac{1}{2}$	4840	3520	2846	10 : 5,9	10 : 8,1	
14	35	65	$19\frac{3}{4}$	$16\frac{1}{2}$	2275	1560	1466	10 : 6,5	10 : 9,4	10:8,5
15	35	120	$21\frac{1}{2}$	$25\frac{1}{2}$	4200	2880	2467	10 : 5,9	10 : 8,6	
16	35	$163\frac{1}{2}$	25	$26\frac{1}{2}$	5728	3924	2981	10 : 5,2	10 : 7,6	
1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.

Observations and Deductions from the foregoing Experiments.

I. Concerning the Ratio between the Power and Effect of Overshot Wheels.

The effective power of the water must be reckoned upon the whole descent; because it must be raised that height, in order to be in a condition of producing the same effect a second time.

The

The ratios between the *powers* ſo eſtimated, and the *effects* at the *maximum* deduced from the ſeveral ſets of experiments, are exhibited at one view in column 9. of Table II.; and from hence it appears, that thoſe ratios differ from that of 10 to 7,6 to that of 10 : 5,2, that is, nearly from 4 : 3 to 4 : 2. In thoſe experiments where the heads of water and quantities expended are leaſt, the proportion is nearly as 4 : 3; but where the heads and quantities are greateſt, it approaches nearer to that of 4 : 2; and by a medium of the whole, the ratio is that of 3 : 2 nearly. We have ſeen before, in our obſervations upon the effects of underſhot wheels, that the general ratio of the power to the effect, when greateſt was 3 : 1; *the effect therefore of overſhot wheels, under the ſame circumſtances of quantity and fall, is at a medium double to that of the underſhot:* and, as a conſequence thereof, *that non elaſtic bodies, when acting by their impulſe or collision, communicate only a part of their original power;* the other part being ſpent in changing their figure in conſequence of the ſtroke.

The powers of water computed from the height of the wheel only, compared with the effects, as in column 10, appear to obſerve a more conſtant ratio: for if we take the medium of each claſs, which is ſet down in column 11, we ſhall find the extremes to differ no more than from the ratio of 10 : 8,1 to that of 10 : 8,5; and as the ſecond term of the ratio gradually increaſes from 8,1 to 8,5, by an increaſe of head from 3 inches to 11, the exceſs of 8,5 above 8,1 is to be imputed to the ſuperior impulſe of the water at the head of 11 inches above that of 3 inches: ſo that if we reduce 8,1 to 8, on account of the impulſe of the 3 inch head, *we ſhall have the ratio of the power computed upon the height of the wheel only, to the effect at a maximum as* 10 : 8, *or as* 5 : 4 *nearly:* and from the quality of the ratio between power and effect ſubſiſting where the conſtructions are ſimilar, we muſt infer, *that the effects,* as well as the powers, *are as the quantities of water and perpendicular heights multiplied together reſpectively.*

II. Concerning the moſt proper Height of the Wheel in Proportion to the whole Deſcent.

We have already ſeen from the preceding obſervation, that the effect of the ſame quantity of water, deſcending through the ſame perpendicular ſpace, is double, when acting by its gravity upon an overſhot wheel, to what the ſame produces when acting by its impulſe upon an underſhot. It alſo appears, that by increaſing the head from 3 inches to 11, that is, the whole deſcent, from 27 inches to 35, or in the ratio of 7 to 9 nearly, the effect is advanced no more than in the ratio of 8,1 to 8,4, that is, as 7 : 7,26; and

conſequently the increaſe of effect as not 1-7th of the increaſe of perpendicular height. Hence it follows, *that the higher the wheel is in proportion to the whole deſcent, the greater will be the effect;* becauſe it depends leſs upon the impulſe of the head, and more upon the gravity of the water in the buckets: and if we conſider how obliquely the water iſſuing from the head muſt ſtrike the buckets, we ſhall not be at a loſs to account for the little advantage that ariſes from the impulſe thereof: and ſhall immediately ſee of how little conſequence this impulſe is to the effect of an overſhot wheel. However, as every thing has its limits, ſo has this: for thus much is deſirable *that the water ſhould have somewhat greater velocity, than the circumference of the wheel, in coming thereon;* otherwiſe the wheel will not only be retarded, by the buckets ſtriking the water, but thereby daſhing a part of it over, ſo much of the power is loſt.

The velocity that the circumference of the wheel ought to have, being known by the following deductions, the head requiſite to give the water its proper velocity is eaſily computed from the common rules of hydroſtatics; and will be found much leſs than what is generally practiſed.

III. Concerning the Velocity of the Circumference of the Wheel, in order to produce the greateſt Effect.

If a body is let fall freely from the ſurface of the head to the bottom of the deſcent, it will take a certain time in falling; and in this caſe the whole action of gravity is ſpent in giving the body a certain velocity: but if this body in falling is made to act upon ſome other body, ſo as to produce a mechanical effect, the falling body will be retarded; becauſe a part of the action of gravity is then ſpent in producing the effect, and the remainder only giving motion to the falling body: and therefore *the ſlower a body deſcends, the greater will be the portion of the action of gravity applicable to the producing a mechanical effect;* and in conſequence the greater that effect may be.

If a ſtream of water falls into the bucket of an overſhot wheel, it is there retained until the wheel by moving round diſcharges it: of conſequence the ſlower the wheel moves, the more water each bucket will receive: ſo that what is loſt in ſpeed, is gained by the preſſure of a greater quantity of water acting in the buckets at once: and if conſidered only in this light, the mechanical power of an overſhot wheel to produce effects will be equal whether it moves quick or ſlow: but if we attend to what has been juſt now obſerved of the falling body, it will appear that ſo much of the action of gravity,

as

as is employed in giving the wheel and water therein a greater velocity, muſt be ſubtracted from its preſſure upon the buckets; ſo that, though the product made by multiplying the number of cubic inches of water acting in the wheel at once by its velocity will be the ſame in all caſes; yet, as each cubic inch, when the velocity is *greater* does not preſs ſo much upon the bucket as when it is *less*, the power of the water to produce effects will be greater in the leſs velocity than in the greater: but hence we are led to this general rule, *that* cæteris paribus, *the less the velocity of the wheel, the greater will be the effect thereof.* A confirmation of this doctrine, together with the limits it is ſubject to in practice, may be deduced from the foregoing ſpecimen of a ſet of experiments.

From theſe experiments it appears that, when the wheel made about 20 turns in a minute, the effect was near upon, the greateſt. When it made 30 turns, the effect was diminiſhed about $\frac{1}{20}$ part; but that when it made 40, it was diminiſhed about $\frac{1}{4}$; when it made leſs than $18\frac{1}{4}$, its motion was irregular; and when it was loaded ſo as not to admit its making 18 turns, the wheel was overpowered by its load.

It is an advantage in practice, that the velocity of the wheel ſhould not be diminiſhed further than what will procure ſome ſolid advantage in point of power: becauſe *cæteris paribus*, as the motion is ſlower, the buckets muſt be made larger; and the wheel being more loaded with water, the ſtreſs upon every part of the work will be increaſed in proportion: *the best velocity for practice therefore will be such, as when the wheel here used made about 30 turns in a minute; that is, when the velocity of the circumference is a little more than 3 feet in a second.*

Experience confirms, that this velocity of 3 feet in a ſecond is applicable to the higheſt overſhot wheels, as well as the loweſt; and all other parts of the work being properly adapted thereto, will produce very nearly the greateſt effect poſſible: however, this alſo is certain from experience, that *high wheels may deviate further from this rule, before they will lose their power, by a given aliquot part of the whole, than low ones can be admitted to do;* for a wheel of 24 feet high may move at the rate of ſix feet per ſecond without loſing any conſiderable part of its power*; and on the other hand, I have ſeen a wheel of 33 feet high, that has moved very ſteadily and well with a velocity but little exceeding 2 feet.

* The 24 feet wheel going at 6 feet in a second, seems owing to the small proportion that the head (requisite to give the water the proper velocity of the wheel) bears to the whole height.

IV. Concerning

IV. Concerning the Load for an Overſhot Wheel, in order that it may produce a Maximum.

The maximum load for an overshot wheel, is that which reduces the circumferences of the wheel to its proper velocity; and this will be known, by dividing the effect it ought to produce in a given time by the ſpace intended to be deſcribed by the circumference of the wheel in the ſame time; the quotient will be the reſiſtance overcome at the circumference of the wheel, and is equal to the load required, the friction and reſiſtance of the machinery included.

V. Concerning the greateſt poſſible Velocity of an Overſhot Wheel.

The greateſt velocity that the circumference of an overſhot wheel is capable of, depends jointly upon the diameter or *height* of the wheel, and the velocity of falling bodies; for it is plain that the velocity of the circumference can never be greater, than to deſcribe a ſemi-circumference, while a body let fall from the top of the wheel will deſcend through its diameter; nor indeed quite ſo great, as a body deſcending through the ſame perpendicular ſpace cannot perform the ſame in ſo ſmall a time when paſſing through a ſemi-circle, as would be done in a perpendicular line. Thus, if a wheel is 16 feet 1 inch high, a body will fall through the diameter in one ſecond: this wheel therefore can never arrive at a velocity equal to the making one turn in two ſeconds; but, in reality, an overſhot wheel can never come near this velocity; for when it acquires a certain ſpeed, the greateſt part of the water is prevented from entering the buckets; and the reſt, at a certain point of its deſcent, is thrown out again by the centrifugal force. This appears to have been the caſe in the three firſt experiments of the foregoing ſpecimen; but as the velocity, when this begins to happen, depends upon the form of the buckets, as well as other circumſtances, *the utmost velocity of overshot wheels is not to be determined generally:* and, indeed, it is the leſs neceſſary in practice, as it is in this circumſtance incapable of producing any *mechanical effect*, for reaſons already given.

VI. Concerning the greateſt Load that an Overſhot Wheel can overcome.

The greatest load an overshot wheel will overcome, considered abstractedly, is unlimited or infinite: for as the buckets may be of any given capacity, the more the wheel

wheel is loaded, the ſlower it turns; but the ſlower it turns, the more will the buckets be filled with water; and conſequently though the diameter of the wheel and quantity of water expended, are both limited, yet no reſiſtance can be aſſigned, which it is not able to overcome: but in practice we always meet with ſomething that prevents our getting into infiniteſimals; for when we really go to work to build a wheel, the buckets muſt neceſſarily be of ſome given capacity; and conſequently *such a resistance will stop the wheel, as is equal to the effort of all the buckets in one semi-circumference filled with water.*

The ſtructure of the buckets being given; the quantity of this effort may be aſſigned; but is not of much conſequence to the practice, as in this caſe alſo the wheel loſes its power; for though here be the exertion of gravity upon a given quantity of water, yet being prevented by a counterbalance from moving, is capable of producing no *mechanical effect*, according to our definition. But, in reality, an overſhot wheel generally ceaſes to be uſeful before it is loaded to that pitch; for *when it meets with such a resistance as to diminish its velocity to a certain degree, its motion becomes irregular; yet this never happens until the velocity of the circumference is less than 2 feet per second, where the resistance is equable*, as appears not only from the preceding ſpecimen, but from experiments on larger wheels.

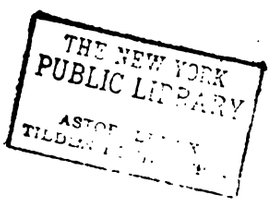
Scholium.

Having now examined the different effects of the power of water, when acting by its *impulse*, and by its *weight*, under the titles of *undershot* and *overshot* wheels; we might naturally proceed to examine the effects when the impulſe and weight are combined, as in the ſeveral kinds of *breast wheels*, &c. but, what has been already delivered being carefully attended to, the application of the ſame principles in theſe mixt caſes will be eaſy, and reduce what I have to ſay on this head into a narrow compaſs: for all kinds of wheels where the water cannot deſcend through a given ſpace, unleſs the wheel moves therewith, are to be conſidered of the nature of an overſhot wheel, according to the perpendicular height that the water deſcends from; and all thoſe that receive the impulſe or ſhock of the water, whether in an horizontal, perpendicular, or oblique direction, are to be conſidered as underſhots. And therefore a wheel which the water ſtrikes at a certain point below the ſurface of the head, and after that deſcends in the arch of a circle, preſſing by its gravity upon the wheel; *the effect of such a wheel will be equal to the effect of an undershot, whose head is equal to the difference of level between the*

the ſurface of the water in the reſervoir and [the point where it ſtrikes the wheel, added to that of an overſhot, whoſe height is equal to the difference of level, between the point where it ſtrikes the wheel and the level of the tail-water. It is here ſuppoſed, that the wheel receives the ſhock of the water at right angles to its radii; and that the velocity of its circumference is properly adapted to receive the utmoſt advantage of both theſe powers; otherwiſe a reduction muſt be made on that account.

Many obvious and conſiderable improvements upon the common practice naturally offer themſelves, from a due conſideration of the principles here eſtabliſhed, as well as many popular errors ſhew themſelves in view: but as my preſent purpoſe extends no farther than the laying down ſuch general rules as will be found to anſwer in practice, I leave the particular application to the intelligent artiſt, and to the curious in theſe matters.

PART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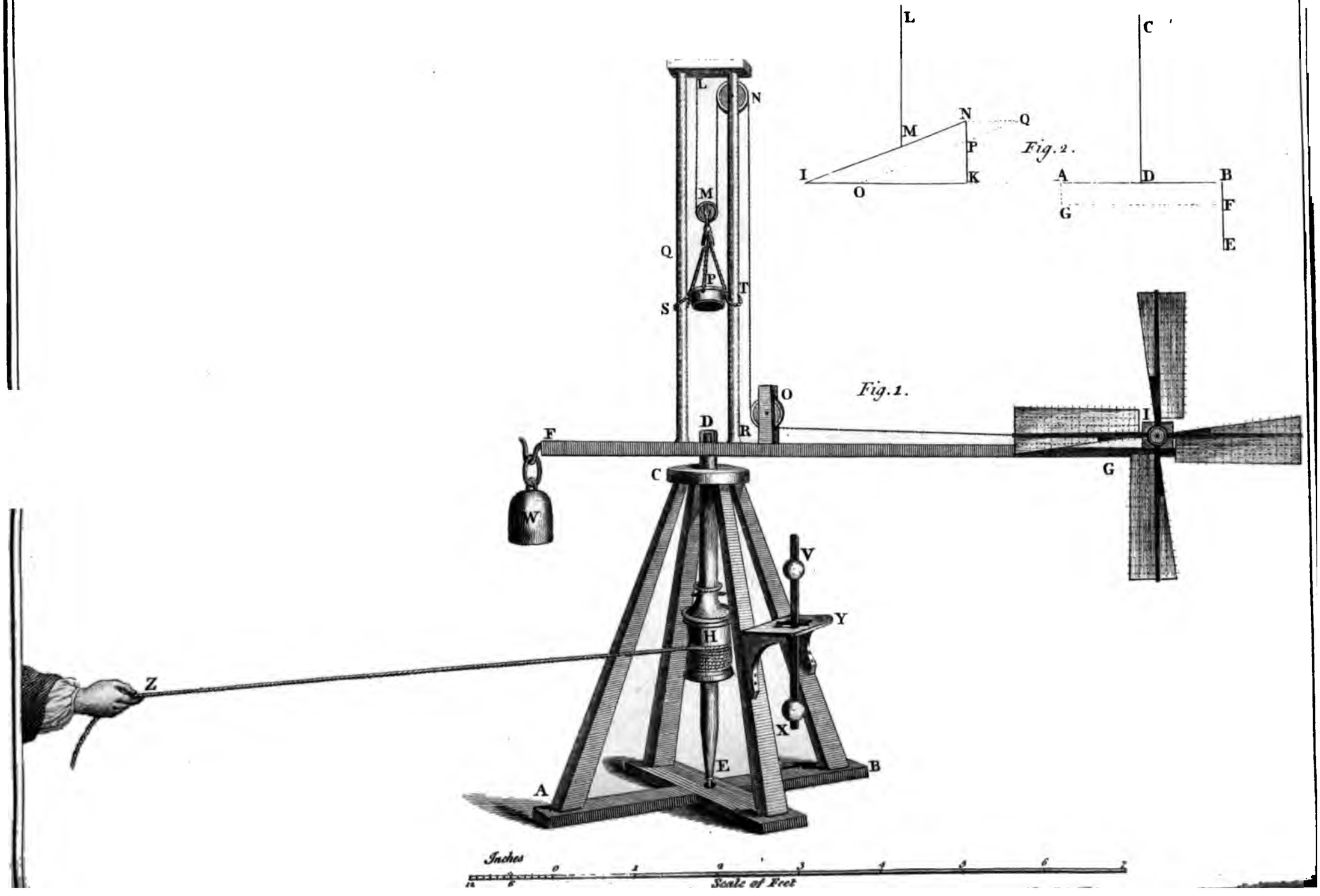
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Fig. 1.

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Fig. 2.

PART III.

On the Construction and Effects of Windmill Sails.

Read before the Royal Society, 31ſt May and 14th June, 1759.

IN trying experiments on wind mill ſails, the wind itſelf is too uncertain to anſwer the purpoſe; we muſt therefore have recourſe to an artificial wind.

This may be done two ways; either by cauſing the air to move againſt the machine, or the machine to move againſt the air. To cauſe the air to move againſt the machine, in a ſufficient volume, with ſteadineſs and the requiſite velocity, is not eaſily put in practice: to carry the machine forward in a right line againſt the air, would require a larger room than I could conveniently meet with. What I found moſt practicable, therefore, was to carry the axis, whereon the ſails were to be fixed, progreſſively round in the circumference of a large circle. Upon this idea* a machine was conſtructed, as follows:

PLATE VI. Fig. 1.

A B C is a pyramidical frame for ſupporting the moving parts.

D E is an upright axis, whereon is framed

F G, an arm for carrying the ſails at a proper diſtance from the centre of the upright axis.

---

* Some years ago Mr. Rouse, an ingenious gentleman of Harborough, in Leicestershire, set about trying experiments on the velocity of the wind, and force thereof upon plain surfaces and wind mill sails: and, much about the same time, Mr. Ellicott contrived a machine for the use of the late celebrated Mr. B. Robins, for trying the resistance of plain surfaces moving through the air. The machines of both these gentlemen were much alike, though at that time totally unacquainted with each other's inquiries. But it often happens, that when two persons think justly upon the same subject, their experiments are alike. This machine was also built upon the same idea as the foregoing; but differed in having the hand for the first mover, with a pendulum for its regulator, instead of a weight, as in the former; which was certainly best for the purposes of measuring the impulse of the wind, or resistance of plains: but the latter is more applicable to experiments on windmill sails; because every change of position of the same sails will occasion their meeting the air with a different velocity, though urged by the same weight.

H is

H is a barrel upon the upright axis, whereon is wound a cord; which, being drawn by the hand, gives a circular motion to the axis, and to the arm F G; and thereby carries the axis of the ſails in the circumference of a circle, whoſe radius is D I, cauſing thereby the ſails to ſtrike the air, and turn round upon their own axis.

At L is fixed the end of a ſmall line, which paſſing through the pullies M N O, terminates upon a ſmall cylinder or barrel upon the axis of the ſails, and, by winding thereon raiſes

P the ſcale, wherein the weights are placed for trying the power of the ſails. This ſcale, moving up and down in the direction of the upright axis, receives no diſturbance from the circular motion.

Q R two parallel pillars ſtanding upon the arm F G, for the purpoſe of ſupporting and keeping ſteady the ſcale P; which is kept from ſwinging by means of

S T, two ſmall chains, which hang looſely round the two pillars.

W is a weight for bringing the centre of gravity of the moveable part of the machine into the centre of motion of the axis D E.

V X is a pendulum, compoſed of two balls of lead, which are moveable upon a wooden rod, and thereby can be ſo adjuſted, as to vibrate in any time required. This pendulum hangs upon a cylindrical wire, whereon it vibrates, as on a rolling axis.

Y is a perforated table for ſupporting the axis of the pendulum.

Note, The pendulum being ſo adjuſted, as to make two vibrations in the time that the arm F G is intended to make one turn; the pendulum being ſet a vibrating, the experimenter pulls by the cord Z, with ſufficient force to make each half revolution of the arm to correſpond with each vibration, as equal as poſſible during the number of vibrations that the experiment is intended to be continued. A little practice renders it eaſy to give motion thereto with all the regularity that is neceſſary.

Specimen

Specimen of a Set of Experiments.

Radius of the ſails - - - - - -	21 inches.
Length of ditto in the cloth - - - -	18
Breadth of ditto - - - - - - -	5,6
* Angle at the extremity - - - -	10 degrees.
* Ditto at the greateſt inclination - - -	25
20 turns of the ſails raiſed the weight - -	11,3
Velocity of the centre of the ſails, in the circumference of the great circle, in a ſecond - - - - - - -	6 feet 0 inches.
Continuance of the experiment - - -	52 ſeconds.

No.	Weight in the ſcale.	Turns.	Product.
1	0 *lb.*	108	0
2	6	85	510
3	6½	81	526½
4	7	78	546
5	7½	73	547½ maximum.
6	8	65	520
7	9	0	0

N. B. The weight of the ſcale and pulley was 3 oz.; and that 1 oz. ſuſpended upon one of the radii, at 1½ inches from the centre of the axis, juſt overcame the friction, ſcale and load of 7½ lb.; and placed at 14 $\frac{10}{16}$ inches, overcame the ſame reſiſtances with 9 lb. in the ſcale.

Reduction of the preceding Specimen.

No. 5, being taken for the maximum, the weight in the ſcale was 7 lb. 8 oz. which, with the weight of the ſcale and pulley 3 oz. makes 7 lb. 11 oz. equal to 123 oz.;

* In all the following experiments the angle of the ſails is accounted from the plane of their motion; that is, when they ſtand at right angles to the axis, their angle is denoted 0°, this notation being agreeable to the language of practitioners, who call the angle ſo denoted, the weather of the ſail; which they denominate greater or leſs, according to the quantity of this angle.

this, added to the friction of the machinery, the ſum is the whole reſiſtance *. The friction of the machinery is thus deduced: ſince 20 turns of the ſails raiſed the weight 11,3 inches, with a double line, the radius of the cylinder will be .18 of an inch; but had the weight been raiſed by a ſingle line, the radius of the cylinder being half the former, viz. .09, the reſiſtance would have been the ſame: we ſhall therefore have this analogy; as half the radius of the cylinder, is to the length of the arm where the ſmall weight was applied; ſo is the weight applied to the arm, to a fourth weight, which is equivalent to the ſum of the whole reſiſtance together; that is, .09 : 12,5 :: 1 oz. : 139 oz. this exceeds 123 oz. the weight in the ſcale, by 16 oz. or 1 lb. which is equivalent to the friction; and which, added to the above weight of 7 lb. 11 oz. makes 8lb. 11 oz. = 8,69 lb. for the ſum of the whole reſiſtance; and this, multiplied by 73 turns, makes a product of 634, which may be called the repreſentative of the *effect* produced.

In like manner, if the weight 9 lb. which cauſed the ſails to reſt after being in motion, be augmented by the weight of the ſcale and its relative friction, it will become 10,37 lb. The reſult of this ſpecimen is ſet down in No. 12, of Table III. and the reſult of every other ſet of experiments therein contained were made and reduced in the ſame manner.

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* The resistance of the air is not taken into the account of resistance, because it is inseparable from the application of the power.

TABLE

TABLE III.

Containing Nineteen Sets of Experiments on Windmill Sails of various Structures, Positions, and Quantities of Surfaces.

The kind of fails made uſe of.	Number.	Angle at the extremities.	Greatest angle.	Turns of the sails unloaded.	Turns of ditto at the maximum.	Load at the maximum.	Greatest load.	Product.	Quantity of surface.	Ratio of greatest velocity to the velocity at maximum.	Ratio of greatest load to the load at maximum.	Ratio of surface to the product.
Plain sails at an angle of 55°	1	° 35	° 35	66	42	*lb.* 7,56	*lb.* 12,59	318	*Sq. In.* 404	10:7	10:6	10:7,9
Plain sails weathered according to the common practice.	2	12	12		70	6,3	7,56	441	404		10:8,3	10:10,1
	3	15	15	105	69	6,72	8,12	464	404	10:6,6	10:8,3	10:10,15
	4	18	18	96	66	7,0	9,81	462	404	10:7,	10:7,1	10:10,15
Weathered according to *Maclaurin's theorem.*	5	9	$26\frac{1}{7}$		66	7,0		462	404			10:11,4
	6	12	$29\frac{1}{2}$		70^{1}	7,35		518	404			10:12,8
	7	15	$32\frac{1}{3}$		$63\frac{1}{3}$	8,3		527	404			10:13,
Sails weathered in the *Dutch* manner, tried in various positions.	8	0	15	120	93	4,75	5,31	442	404	10:7,7	10:8,9	10:11,
	9	3	18	120	79	7,0	8,12	553	404	10:6,6	10:8,6	10:13,7
	10	5	20		78	7,5	8,12	585	404		10:9,2	10:14,5
	11	$7\frac{1}{2}$	$22\frac{5}{8}$	113	77	8,3	9,81	639	404	10:6,8	10:8,5	10:15,8
	12	10	25	108	73	8,69	10,37	634	404	10:6,8	10:8,4	10:15,7
	13	12	27	100	66	8,41	10,94	580	404	10:6,6	10:7,7	10:14,4
Sails weathered in the *Dutch* manner, but *enlarged* towards the extremities.	14	$7\frac{1}{2}$	$22\frac{1}{2}$	123	75	10,65	12,59	799	505	10:6,1	10:8,5	10:15.8
	15	10	25	117	74	11,08	13,69	820	505	10:6,3	10:8,1	10:16,2
	16	12	27	114	66	12,09	14,23	799	505	10:5,8	10:8,4	10:15,8
	17	15	30	96	63	12,09	14,78	762	505	10:6,6	10:8,2	10:15,1
Eight sails being *sectors* of *ellipses* in their best positions.	18	12	22	105	$64\frac{1}{2}$	16,42	27,87	1059	854	10:6,1	10:5,9	10:12,4
	19	12	22	99	$64\frac{1}{2}$	18,06		1165	1146	10:5,9		10:10,1
	1.	2.	3.	4.	5.	6.	7.	8.	9.	10.	11.	12.

Observations

Observations and Deductions from the preceding Experiments.

I. Concerning the beſt Form and Poſition of Windmill-Sails.

In Table III. N°. 1. is contained the reſult of a ſet of experiments upon ſails ſet at the angle which the celebrated Monſ. Parint, and ſucceeding geometricians for many years, held to be the beſt; *viz.* thoſe whoſe planes make an angle 55° nearly with the axis; the complement whereof, or angle that the plane of the ſail makes with the plane of their motion, will therefore be 35°, as ſet down in columns 2. and 3. Now if we multiply their number of turns by the weight they lifted, when working to the greateſt advantage, as ſet down in columns 5. and 6. and compare this product (col. 8.) with the other products contained in the ſame column, inſtead of being the greateſt, it turns out the leaſt of all the reſt. But if we ſet the angle of the ſame planes at ſomewhat leſs than half the former, or at any angle from 15° to 18°, as in N°. 3. and 4. that is, from 72° to 75° with the axis, the product will be increaſed in the ratio of 31 : 45; and this is the angle moſt commonly made uſe of by practioners, when the ſurfaces of the ſails are planes.

If nothing more was intended than to determine the moſt efficacious angle to make a mill acquire motion from a ſtate of reſt, or to prevent it from paſſing into reſt from a ſtate of motion, we ſhall find the poſition of N°. 1. the beſt; for if we conſult col. 7. which contains the leaſt weights, that would make the ſails paſs from motion to reſt, we ſhall find that of N°. 1. (relative to the quantity of cloth) the greateſt of all. But if the ſails are intended, with given dimenſions, to produce the greateſt effect poſſible in a given time, we muſt entirely reject thoſe of N°. 1. and, *if we are confined to the use of planes, conform ourselves to some angle between* N°. 3. *and* 4. *that is, not less than* 72°, *or greater than* 75, *with the axis.*

The late celebrated Mr. Maclaurin has judiciouſly diſtinguiſhed between the action of the wind upon a ſail at reſt, and a ſail in motion; and, in conſequence, as the motion is more rapid near the extremities than towards the centre, that the angle of the different parts of the ſail, as they recede from the centre, ſhould be varied. For this purpoſe he has furniſhed us with the following theorem *. "Suppoſe the velocity of the wind to "be repreſented by *a*, and the velocity of any given part of the ſail to be denoted by *c*;

* Maclaurin's account of Sir Isaac Newton's philosophical discoveries, p. 176, art. 29.

"then

" then the effort of the wind upon that part of the ſail will be greateſt when the tangent " of the angle, in which the wind ſtrikes it, is to radius as

" $\sqrt{2+\frac{9cc}{4aa}+\frac{3c}{2a}}$ to 1." This theorem then aſſigns the law, by which the angle is to be varied according to the velocity of each part of the ſail to the wind: but as it is left undetermined what velocity any one given part of the ſail ought to have in reſpect to the wind, the angle that any one part of the ſail ought to have, is left undetermined alſo; ſo that we are ſtill at a loſs for the proper *data* to apply the theorem. However, being willing to avail myſelf thereof, and conſidering that any angle from 15° to 18° was beſt ſuited to a plane, and of conſequence to the beſt mean angle, I made the ſail, at the middle diſtance between the centre and the extremity, to ſtand at an angle of 15° 41′ with the plane of the motion: in which caſe the velocity of that part of the ſail, when loaded to a *maximum*, would be equal to that of the wind, or $c=a$. This being determined, the reſt were inclined according to the theorem, as follows:

		Angle with the axis.	Angle of weather.	
Parts of the radius from the centre.	$\frac{5}{6}$ - - $c=\frac{1}{3}a$	63° 26′	26° 34′	
	$\frac{4}{6}$ - - $c=\frac{2}{3}a$	69 54	20 6	
	$\frac{3}{6}$ - - $c=a$	74 19	15 41	middle.
	$\frac{2}{3}$ - - $c=1\frac{1}{3}a$	77 20	12 40	
	$\frac{5}{6}$ - - $c=1\frac{2}{3}a$	79 27	10 33	
	1 - - $c=2a$	81 0	9 0	extremity.

The reſult hereof was according to N°. 5, being nearly the ſame as the plane ſails, in their beſt poſition: but being turned round in their ſockets, ſo that every part of each ſail ſtood at an angle of 3°, and afterwards of 6°, greater than before, that is, their extremities being moved from 9° to 12° and 15°, the products were advanced to 518 and 527 reſpectively. Now from the ſmall difference between thoſe two products, we may conclude, that they were nearly in their beſt poſition, according to N°. 7, or ſome angle between that and N°. 6: but from theſe as well as the plane ſails and others, we may alſo conclude, that *a variation in the angle of a degree or two makes very little difference in the effect, when the angle is near upon the beſt.*

It is to be obſerved, that a ſail inclined by the preceding rule will expoſe a convex ſurface to the wind: whereas the Dutch, and all our modern mill-builders, though they

make the angle to diminiſh, in receding from the centre towards the extremity, yet conſtantly do it in ſuch manner, as that the ſurface of the ſail may be concave towards the wind. In this manner the ſails made uſe of in N°. 8, 9, 10, 11, 12, and 13, were conſtructed; the middle of the ſail making an angle with the extreme bar of 12°; and the greateſt angle (which was about $\frac{1}{7}$ of the radius from the centre) of 15° therewith. Thoſe ſails being tried in various poſitions, the beſt appears to be that of N°. 11, where the extremities ſtood at an angle of 7°$\frac{1}{2}$ with the plane of motion, the product being 639: greater than that of thoſe made by the theorem in the ratio of 9 : 11, and double to that of N°. 1; and this was the greateſt product that could be procured without an augmentation of ſurface. Hence it appears, that *when the wind falls upon a concave ſurface, it is an advantage to the power of the whole, though every part, taken ſeparately, ſhould not be diſpoſed to the beſt advantage**.

Having thus obtained the beſt poſition of the ſails, or manner of weathering, as it is called by the workmen, the next point was to try what advantage could be made by an addition of ſurface upon the ſame radius. For this purpoſe, the ſails made uſe of had the ſame weather as thoſe N°. 8 to 13, with an addition to the leading ſide of each of a triangular cloth, whoſe height was equal to the height of the ſail, and whoſe baſe was equal to half the breadth: of conſequence the increaſe of ſurface upon the whole was one-fourth part, or as 4 : 5. Thoſe ſails, by being turned round in their ſockets, were tried in four different poſitions, ſpecified in N°. 14, 15, 16, and 17; from whence it appears, that the beſt was when every part of the ſail made a greater angle by 2°$\frac{1}{2}$, with the plane of the motion, than thoſe without the addition, as appears by N° 15, the product being 820: this exceeds 639 more than in the ratio of 4 : 5, or that of the increaſe of cloth. Hence it appears, that *a broader ſail requires a*

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* By ſeveral trials in large I have found the following angles to anſwer as well as any. The radius is ſupposed to be divided into ſix parts and one-ſixth, reckoning from the centre, is called one, the extremity being denoted ſix.

N°.	Angle with the axis.	Angle with the plane of the motion.
1	72°	18°
2	71	19
3	72	18 middle.
4	74	16
5	77$\frac{1}{2}$	12$\frac{1}{2}$
6	83	7 extremity.

greater

greater angle; and *that when the sail is broader at the extremity, than near the centre, this shape is more advantageous than that of a parallelogram* *

Many have imagined, that the more ſail, the greater the advantage, and have therefore propoſed to fill up the whole area: and by making each ſail a ſector of an ellipſis, according to Monſieur Parint, to intercept the whole cylinder of wind, and thereby to produce the greateſt effect poſſible.

We have therefore proceeded to inquire how far the effect could be increaſed by a further enlargement of the ſurface, upon the ſame radius of which N°. 18 and 19 are ſpecimens. The ſurfaces indeed were not made planes, and ſet at an angle of 35°, as Parint propoſed; becauſe, from N°. 1, we learn, that this poſition has nothing to do, when we intend them to work to the greateſt advantage. We therefore gave them ſuch an angle as the preceding experiments indicated for ſuch ſort of ſails, viz. 12° at the extremity, and 22° for the greateſt weather. By N°. 18 we have the product 1059, greater than N°. 15, in the ratio of 7 : 9; but then the augmentation of cloth is almoſt 7 : 12. By N°. 19, we have the product 1165, that is greater than N°. 15, as 7 : 10; but the augmentation of cloth is nearly as 7 : 16; conſequently had the ſame quantity of cloth as in N°. 18, been diſpoſed in a figure ſimilar to that of N°. 15, inſtead of the product 1059, we ſhould have had the product 1386; and in N°. 19, inſtead of the product 1165, we ſhould have had a product of 1860; as will be further made appear in the courſe of the following deductions. Hence it appears, that beyond a certain degree, the more the area is crouded with ſail, the leſs effect is produced in proportion to the ſurface: and, by purſuing the experiments ſtill further, I found, that though in N°. 19, the ſurface of all the ſails together were not more than ſeven-eighths of the circular area containing them, yet a further addition rather diminiſhed than increaſed the effect. *So that when the whole cylinder of wind is intercepted, it does not then produce the greatest effect for want of proper interstices to escape.*

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* The figure and proportion of the enlarged sails, which I have found best to answer in large, are represented in the figure, Plate VI. where the extreme bar is one-third of the radius (or whip, as it is called by the workmen), and is divided by the whip in the proportion of 3 to 5. The triangular or leading sail is covered with board from the point downwards one-third of its height, the rest with cloth as usual. The angles of weather in the preceding note are best for the enlarged sails also; for in practice it is found, that the sails had better have too little than much weather.

It

It is certainly desirable that the sails of windmills should be as short as possible; but at the same time it is equally desirable, the quantity of cloth should be the least that may be, to avoid damage by sudden squalls of wind. The best structure, therefore, for large mills, is that where the quantity of cloth is the greatest, in a given circle, that can be: on this condition, that the effect holds out in proportion. to the quantity of cloth; for otherwise the effect can be augmented in a given degree by a lesser increase of cloth upon a larger radius, than would be required, if the cloth was increased upon the same radius. The most useful figure therefore for practice, is that of N°. 9 or 10, as has been experienced upon several mills in large.

TABLE IV.

Containing the Result of six Sets of Experiments, made for determining the Difference of Effect, according to the different Velocity of the Wind.

N. B. The sails were of the same size and kind as those of N°. 10, 11, and 12, Table III.

Continuance of the Experiment one Minute.

Number.	Angle at the extremity.	Velocity of the wind in a second.	Turns of the sails unloaded.	Turns of the sails at maximum.	Load at the maximum.	Greatest load.	Product.	Maximum load for the half velocity.	Turns of the sails therewith.	Product of lesser load and greater velocity.	Ratio of the two products.	Ratio of the greatest velocity to the velocity at a maxim^m	Ratio of the greatest load to the load at a maximum.
		f. in.			lb.	lb.							
1	5°	4 4½	96	66	4,47	5,37	295	—	—	—	— —	10:6,9	10:8,3
2	5	8 9	207	122	16,42	18,06	2003	4,47	180	805	10 : 27,3	10:5,9	10:9,1
3	7½	4 4½	—	65	4,62	—	300	—	—	—	— —	——	——
4	7½	8 9	—	130	17,52	—	2278	4,62	180	832	10 : 27,8	——	——
5	10	4 4½	91	61	5,03	5,87	307	—	—	—	— —	10:6,7	10:8,5
6	10	8 9	178	110	18,61	21,34	2047	5,03	158	795	10 : 26	10:6,2	10:8,7
1	2	3	4	5	6	7	8	9	10	11	12	13	14

II. *Concerning*

II. Concerning the Ratio between the Velocity of Windmill Sails unloaded, and their Velocity when loaded to a *Maximum*.

Thoſe ratios, as they turned out in experiments upon different kinds of ſails, and with different inclinations (the velocity of the wind being the ſame) are contained in column 10 of Table III. where the extremes differ from the ratio of 10: 7,7 to that of 10 : 5,8; but *the most general ratio of the whole will be nearly as* 3 : 2. This ratio alſo agrees ſufficiently near with experiments where the velocity of the wind was different, as in thoſe contained in Table IV. column 13, in which the ratios differ from 10 : 6,9 to that of 10 : 5,9. However, it appears in general, that where the power is greater, whether by an enlargement of ſurface, or a greater velocity of the wind, that the ſecond term of the ratio is leſs.

III. Concerning the Ratio between the greateſt Load that the Sails will bear without ſtopping, or what is nearly the ſame Thing, between the leaſt Load that will ſtop the Sails, and the Load at the *Maximum*.

Thoſe ratios for different kinds of ſails and inclinations, are collected in column 11, Table III. where the extremes differ from the ratio of 10 : 6 to that of 10 : 9,2; but taking in thoſe ſets of experiments only, where the ſails reſpectively anſwered beſt, *the ratio's will be confined between that of* 10 : 8 *and of* 10 : 9; *and at a medium about* 10 : 8,3 *or of* 6 : 5. This ratio alſo agrees nearly with thoſe in column 14 of Table IV. However it appears, upon the whole, that in thoſe inſtances, where the angle of the ſails or quantity of cloth were greateſt, that the ſecond term of the ratio was leſs.

IV. Concerning the Effects of Sails, according to the different Velocity of the Wind.

Maxim I. The velocity of windmill ſails, whether unloaded, or loaded, ſo as to produce a *maximum*, is nearly as the velocity of the wind, their ſhape and poſition being the ſame.

This appears by comparing together the reſpective numbers of columns 4 and 5, Table IV. wherein thoſe of numbers 2, 4, and 6, ought to be double of numbers 1, 3, and 5: but as the deviation is no where greater than what may be imputed to

the inaccuracy of the experiments themſelves, and hold good exactly in numbers 3 and 4; which ſets were deduced from the medium of a number of experiments, carefully repeated the ſame day, and on that account are moſt to be depended upon; we therefore conclude the maxim true.

Maxim II. The load at the *maximum* is nearly, but ſomewhat leſs than, as the ſquare of the velocity of the wind, the ſhape and poſition of the ſails being the ſame.

This appears by comparing together the numbers in column 6, Table IV. wherein thoſe of numbers 2, 4, and 6, (as the velocity is double), ought to be quadruple of thoſe of numbers 1, 3, and 5; inſtead of which they fall ſhort, number 2 by $\frac{1}{14}$, number 4 by $\frac{1}{10}$, and number 6 by $\frac{1}{11}$ part of the whole. The greateſt of thoſe deviations is not more conſiderable than might be imputed to the unavoidable errors in making the experiments: but as thoſe experiments, as well as thoſe of the greateſt load, all deviate the ſame way; and alſo coincide with ſome experiments communicated to me by Mr. Rouſe upon the reſiſtance of planes; I am led to ſuppoſe a ſmall deviation, whereby the load falls ſhort of the ſquares of the velocity; and ſince the experiments No. 3 and 4, are moſt to be depended upon, we muſt conclude, that when the velocity is double, the load falls ſhort of its due proportion by $\frac{1}{10}$, or, for the ſake of a round number, by about $\frac{1}{10}$ part of the whole.

Maxim III. The effects of the ſame ſails at a *maximum* are nearly, but ſomewhat leſs than, as the cubes of the velocity of the wind.

It has already been proved, Maxim 1ſt, that the velocity of ſails at the *maximum*, is nearly as the velocity of the wind; and by Maxim 2d, that the load at the *maximum* is nearly as the ſquare of the ſame velocity: if thoſe two *maximums* would hold preciſely, it would be a conſequence that the effect would be in a triplicate ratio thereof: how this agrees with experiment will appear by comparing together the products in column 8, of Table IV. wherein thoſe of No. 2, 4, and 6, (the velocity of the wind being double) ought to be octuple of thoſe of No. 1, 3, and 5, inſtead of which they fall ſhort, No. 2 by $\frac{1}{7}$, No. 4 by $\frac{1}{10}$, and No. 6, by $\frac{1}{8}$ part of the whole. Now, if we rely on No. 3 and 4, as the turns of the ſails are as the velocity of the wind; and ſince the load of the *maximum* falls ſhort of the ſquare of the velocity by about $\frac{1}{10}$ part of the whole: the product made by the multiplication of the turns into the load, muſt alſo fall ſhort of the triplicate ratio by about $\frac{1}{10}$ part of the whole product.

Maxim

Maxim IV. The load of the ſame ſails at the *maximum* is nearly as the ſquares, and their effect as the cubes of their number of turns in a given time.

This maxim may be eſteemed a conſequence of the three preceding; for if the turns of the ſails are as the velocity of the wind, whatever quantities are in any given ratio of the velocity of the wind, will be in the ſame given ratio of the turns of the ſails: and therefore, if the load at the *maximum* is as the ſquare, or the effect as the cube, of the velocity of the wind, wanting $\frac{1}{100}$ part when the velocity is double; the load at the *maximum* will alſo be as the ſquare, and the effect as the cube, of the number of turns of the ſails in a given time, wanting in like manner $\frac{1}{100}$ part when the number of turns are double in the ſame time. In the preſent caſe, if we compare the loads at the *maximum*, column 6, with the ſquares of the number of turns, column 5, of No. 1 and 2, 5 and 6, or the products of the ſame numbers column 8, with the cubes of the number of turns column 5, inſtead of falling ſhort, as No. 3 and 4, they exceed thoſe ratios: but as the ſets of experiments No. 1 and 2, of 5 and 6, are not to be eſteemed of equal authority with thoſe of No. 3 and 4, we muſt not rely upon them further than to obſerve, that *in comparing the gross effects of large machines, the direct proportion of the squares and cubes respectively, will hold as near as the effects themselves can be observed;* and therefore be ſufficient for practical eſtimation, without any allowance.

Maxim V. When ſails are loaded ſo as to produce a *maximum* at a given velocity, and the velocity of the wind increaſes, the load continuing the ſame; 1ſt, The increaſe of effect, when the increaſe of the velocity of the wind is ſmall, will be nearly as the ſquares of thoſe velocities: 2dly, When the velocity of the wind is double, the effects will be nearly as 10 : 27½: But, 3dly, When the velocities compared, are more than double of that where the given load produces a *maximum*, the effects increaſe nearly in a ſimple ratio of the velocity of the wind.

It has already been proved, maxim 1ſt and 2d, that when the velocity of the wind is increaſed, the turns of the ſails will increaſe in the ſame proportion, even when oppoſed by a load as the ſquare of the velocity: and therefore if wanting the oppoſition of an increaſe of load, as the ſquare of the velocity, the turns of the ſails will again be increaſed in a ſimple ratio of the velocity of the wind on that account alſo; that is, the load continuing the ſame, the turns of the ſails in a given time will be as the ſquare of the velocity of the wind; and the effect, being in this caſe as the turns of the

the ſails will be as the ſquare of the velocity of the wind alſo; but this muſt be underſtood only of the firſt increments of the velocity of the wind: for,

2dly, As the ſails will never acquire above a given velocity in relation to the wind, though the load were diminiſhed to nothing; when the load continues the ſame, the more the velocity of the wind increaſes (though the effect will continue to increaſe) yet the more it will fall ſhort of the ſquare of the velocity of the wind; ſo that when the velocity of the wind is double, the increaſe of effect, inſtead of being as 1 : 4, according to the ſquares, it turns out as 10 : 27½, as thus appears. In Table IV. column 9, the loads of No. 2, 4, and 6, are the ſame as the *maximum* loads in column 6, of No. 1, 3, and 5. The number of turns of the ſails with thoſe loads, when the velocity of the wind is double, are ſet down in column 10, and the products of their multiplication in column 11: thoſe being compared with the products of No. 1, 3, and 5, column 8, furniſh the ratios ſet down in column 12, which at a medium (due regard being had to No. 3 and 4.) will be nearly as 10 : 27½. 3dly. The load continuing the ſame, grows more and more inconſiderable, reſpecting the power of the wind as it increaſes in velocity; ſo that the turns of the ſails grow nearer and nearer a coincidence with their turns unloaded; that is, nearer and nearer to the ſimple ratio of the velocity of the wind. When the velocity of the wind is double, the turns of the ſails, when loaded to a *maximum*, will be double alſo; but, *unloaded*, will be no more than triple, by deduction 2d: and therefore the product could not have increaſed beyond the ratio of 10 : 30 (inſtead of 10 : 27½) even ſuppoſing the ſails not to have been retarded at all by carrying the *maximum* load for the half velocity. Hence we ſee, that when the velocity of the wind exceeds the double of that, where a conſtant load produces a *maximum*, that the increaſe of effect, which follows the increaſe of the velocity of the ſails, will be nearly as the velocity of the wind, and ultimately in that ratio preciſely. Hence alſo we ſee that windmills, ſuch as the different ſpecies for raiſing water for drainage, &c. loſe much of their full effect, when acting againſt one invariable oppoſition.

V. Concerning the Effects of Sails of different Magnitudes the Structure and Poſition being ſimilar, and the Velocity of the Wind the ſame.

Maxim 6. In ſails of a ſimilar figure and poſition, the number of turns in a given time will be reciprocally as the radius or length of the ſail.

The

The extreme bar having the ſame inclination to the plane of its motion, and to the wind; its velocity at a *maximum* will always be in a given ratio to the velocity of the wind; and therefore, whatever be the radius, the abſolute velocity of the extremity of the ſail will be the ſame: and this will hold good reſpecting any other bar, whoſe inclination is the ſame, at a proportionable diſtance from the centre; it therefore follows, that the extremity of all ſimilar ſails, with the ſame wind, will have the ſame abſolute velocity; and therefore take a ſpace of time to perform one revolution in proportion to the radius; or, which is the ſame thing, the number of revolutions in the ſame given time, will be reciprocally as the length of the ſail.

Maxim 7. The load at a *maximum* that ſails of a ſimilar figure and poſition will overcome, at a given diſtance from the centre of motion, will be as the cube of the radius.

Geometry informs us, that in ſimilar figures the ſurfaces are as the ſquares of their ſimilar ſides; of conſequence the quantity of cloth will be as the ſquare of the radius: alſo in ſimilar figures and poſitions, the impulſe of the wind, upon every ſimilar ſection of the cloth, will be in proportion to the ſurface of that ſection; and conſequently, the impulſe of the wind upon the whole, will be as the ſurface of the whole: but as the diſtance of every ſimilar ſection, from the centre of motion, will be as the radius; the diſtance of the centre of power of the whole, from the centre of motion, will be as the radius alſo; that is, the lever by which the power acts, will be as the radius: as therefore the impulſe of the wind, reſpecting the quantity of cloth, is as the ſquare of the radius, and the lever, by which it acts, as the radius ſimply; it follows, that the load which the ſails will overcome, at a given diſtance from the centre, will be as the cube of the radius.

Maxim 8. The effect of ſails of ſimilar figure and poſition, are as the ſquare of the radius.

By maxim 6. it is proved, that the number of revolutions made in a given time, are as the radius inverſely. Under maxim 7. it appears, that the length of the lever, by which the power acts, is as the radius directly; therefore theſe equal and oppoſite ratios deſtroy one another: but as in ſimilar figures the quantity of cloth is as the ſquare of the radius, and the action of the wind is in proportion to the quantity of cloth, as alſo appears under maxim 7; it follows that the effect is as the ſquare of the radius.

COROL.

Corol. 1. Hence it follows, that augmenting the length of the ſail, without augmenting the quantity of cloth, does not increaſe the power; becauſe what is gained by the length of the lever, is loſt by the flowneſs of the rotation.

Corol. 2. If ſails are increaſed in length, the breadth remaining the ſame, the effect will be as the radius.

VI. Concerning the Velocity of the Extremities of Windmill Sails, in reſpect to the Velocity of the Wind.

Maxim 9. The velocity of the extremities of Dutch ſails, as well as of the enlarged ſails, in all their uſual poſitions when unloaded, or even loaded to a *maximum*, are conſiderably quicker than the velocity of the wind.

The Dutch ſails unloaded, as in Table III. No. 8. made 120 revolutions in 52″: the diameter of the ſails being 3 feet 6 inches, the velocity of their extremities will be 25,4 feet in a ſecond; but the velocity of the wind producing it, being 6 feet in the ſame time, we ſhall have 6 : 25,4 :: 1 : 4,2; in this caſe therefore, the velocity of their extremities was 4,2 times greater than that of the wind. In like manner, the relative velocity of the wind, to the extremities of the ſame ſails, when loaded to a *maximum*, making then 93 turns in 52″, will be found to be as 1 : 3,3; or 3,3 times quicker than that of the wind.

The following table contains 6 examples of Dutch ſails, and 4 examples of the enlarged ſails, in different poſitions, but with the conſtant velocity of the wind of 6 feet in a ſecond, from Table III.: and alſo 6 examples of Dutch ſails in different poſitions, with different velocities of the wind from Table IV.

TABLE

TABLE V.

Containing the Ratio of the Velocity of the Extremities of Windmill Sails to the Velocity of the Wind.

Number.	No. of Table III. and IV.	Angle at the Extremity.	Velocity of the Wind in a Second.	Ratio of the Velocity of the Wind, and Extremities of the Sails.		
				unloaded.	loaded.	
1	8	0°	6f 0in	1 : 4,2	1 : 3,3	From Table III.
2	9	3	6 0	1 : 4,2	1 : 2,8	
3	10	5	6 0	— —	1 : 2,75	
4	11	7	6 0	1 : 4,	1 : 2,7	
5	12	10	6 0	1 : 3,8	1 : 2,6	
6	13	12	6 0	1 : 3,5	1 : 2,3	
7	14	7½	6 0	1 : 4,3	1 : 2,6	
8	15	10	6 0	1 : 4,1	1 : 2,6	
9	16	12	6 0	1 : 4,	1 : 2,3	
10	17	15	6 0	1 : 3,35	1 : 2,2	
11	1	5	4 4½	1 : 4,	1 : 2,8	From Tab. IV.
12	2	5	8 9	1 : 4,3	1 : 2,6	
13	3	7½	4 4½	— —	1 : 2,8	
14	4	7½	8 9	— —	1 : 2,7	
15	5	10	4 4½	1 : 3,8	1 : 2,6	
16	6	10	8 9	1 : 3,4	1 : 23	
1	2	3	4	5	6	

It appears from the preceding collection of examples, that when the extremities of the Dutch sails are parallel to the plane of motion, or at right angles to the wind, and to the axis, as they are made according to the common practice in England, that their velocity, unloaded, is above 4 times, and loaded to a *maximum*, above 3 times greater than that of the wind: but that when the Dutch sails, or enlarged sails, are in their best positions, their velocity unloaded is 4 times; and loaded to a *maximum*, at a medium the

Dutch

Dutch ſails are 2,7, and the enlarged ſails 2,6 times greater than the velocity of the wind. Hence we are furniſhed with a method of knowing the velocity of the wind, from obſerving the velocity of the windmill ſails; for knowing the radius, and the number of turns in a minute, we ſhall have the velocity of the extremities; which, divided by the following diviſors, will give the velocity of the wind.

Dutch ſails in the common poſition	unloaded	4.2
	loaded	—3.3
Dutch ſails in their beſt poſition	unloaded	4.0
	loaded	—2.7
Enlarged ſails in their beſt poſition	unloaded	4.0
	loaded	—2.6

From the above diviſors there ariſe the following compendiums; ſuppoſing the radius to be 30 feet, which is the moſt uſual length in this country, and the mill to be loaded to a *maximum*, as is uſually the caſe with corn mills; for every 3 ***turns in a minute, of the Dutch sails in their common position, the wind will move at the rate of*** 2 ***miles an hour;*** for every 5 ***turns in a minute, of the Dutch sails in their best position, the wind moves*** 4 ***miles an hour;*** and for every 6 ***turns in a minute, of the enlarged sails in their best position, the wind will move*** 5 ***miles an hour.***

The following table was communicated to me by my friend Mr. Rouſe, and appears to have been conſtructed with great care, from a conſiderable number of facts and experiments; and having relation to the ſubject of this article, I here inſert it as he ſent it to me: but at the ſame time muſt obſerve, that the evidence for thoſe numbers where the velocity of the wind exceeds 50 miles an hour, does not ſeem of equal authority with that of 50 miles an hour and under. It is alſo to be obſerved, that the numbers in column 3. are calculated according to the ſquare of the velocity of the wind, which, in moderate velocities, from what has been before obſerved, will hold very nearly.

TABLE

TABLE VI.

Containing the Velocity and Force of Wind, according to their common Appellations.

Velocity of the Wind.		Perpendicular force on one foot area in pounds avoirdupois.	Common appellations of the force of winds.
Miles in one Hour.	Feet in one ſecond.		
1	1,47	,005	Hardly perceptible.
2	2,93	,020	Juſt perceptible.
3	4,40	,044	
4	5,87	,079	Gentle pleaſant wind.
5	7,33	,123	
10	14,67	,492	Pleaſant briſk gale.
15	22,00	1,107	
20	29,34	1,968	Very briſk.
25	36,67	3,075	
30	44,01	4,429	High winds.
35	51,34	6,027	
40	58,68	7,873	Very high.
45	66,01	9,963	
50	73,35	12,300	A ſtorm or tempeſt.
60	88,02	17,715	A great ſtorm.
80	117,36	31,490	An hurricane.
100	146,70	49,200	An hurricane that tears up trees, carries buildings before it, &c.
1	2	3	

VII. Concerning the abſolute Effect, produced by a given Velocity of the Wind, upon Sails of a given Magnitude and Conſtruction.

It has been obſerved by practitioners, that in mills with Dutch ſails in the common poſition, when they make about 13 turns in a minute, they then work at a mean

rate: that is, by the compendiums in the laſt article, when the velocity of the wind is $8\frac{2}{3}$ miles an hour, or $12\frac{2}{3}$ feet in a ſecond; which, in common phraſe, would be called a *freſh gale*.

The experiments ſet down in Table IV. No. 4, were tried with a wind, whoſe velocity was $8\frac{1}{4}$ feet in a ſecond; conſequently had thoſe experiments been tried with a wind, whoſe velocity was $12\frac{2}{3}$ feet in a ſecond, the effect, by maxim 3d, would have been 3 times greater; becauſe the cube of $12\frac{2}{3}$ is 3 times greater than that of $8\frac{1}{4}$.

From Table IV. No. 4, we find that the ſails, when the velocity of the wind was $8\frac{1}{4}$ feet in a ſecond, made 130 revolutions in a minute, with a load of 17,52 lb. From the meaſures of the machine, preceding the ſpecimen of a ſet of experiments, we find, that 20 revolutions of the ſails raiſed the ſcale and weight 11,3 inches: 130 revolutions will therefore raiſe the ſcale 73,45 inches, which, multiplied by 17,52 lb. makes a product of 1287, for the effect of the Dutch ſails in their beſt poſition; that is, when the velocity of the wind is $8\frac{1}{4}$ feet in a ſecond: this product therefore multiplied by three, will give 3861 for the effect of the ſame ſails, when the velocity of the wind is $12\frac{2}{3}$ feet in a ſecond.

Deſaguliers makes the utmoſt power of a man, when working ſo as to be able to hold it for ſome hours, to be equal to that of raiſing an hogſhead of water 10 feet high in a minute. Now, an hogſhead conſiſting of 63 ale gallons, being reduced into pounds avoirdupois, and the height into inches; the product made by multiplying thoſe two numbers will be 76800; which is 19 times greater than the product of the ſails laſt mentioned, at $12\frac{1}{2}$ feet in a ſecond: therefore, by maxim 8th, if we multiply the ſquare root of 19, that is 4,46, by 21 inches, the length of the ſail producing the effect 3861, we ſhall have 93,66 inches, or 7 feet $9\frac{2}{3}$ inches for the radius of a Dutch ſail in its beſt poſition, whoſe mean power ſhall be equal to that of a man: but if they are in their common poſition, their length muſt be increaſed in the ratio of the ſquare root of 442 to that of 639, as thus appears;

The ratio of the *maximum* products of No. 8 and 11, Table III. are as 442 : 639: but by maxim 8, the effects of ſails of different radii are as the ſquare of the radii; conſequently the ſquare roots of the products or effects, are as the radii ſimply; and therefore as the ſquare root of 442 is to that of 639; ſo is 93,66 to 112,66; or 9 feet $4\frac{2}{3}$ inches.

If

If the ſails are of the enlarged kind, then from Table III. No. 11 and 15, we ſhall have the ſquare root of 820 to that of 639 : : 93,66 : 82,8 inches, or 6 feet 10¾ inches: ſo that in round numbers we ſhall have the radius of a ſail, of ſimilar figure to their reſpective models, whoſe mean power ſhall be equal to that of a man;

The Dutch ſails in their common poſition	9½ feet.
The Dutch ſails in their beſt poſition	8
The enlarged ſails in their beſt poſition	7

Suppoſe now the radius of a ſail to be 30 feet, and to be conſtructed upon the model of the enlarged ſails, No. 14 or 15, Table III. dividing 30 by 7, we ſhall have 4,28, the ſquare of which is 18,3; and this, according to maxim 7, will be the relative power of a ſail of 30 feet, to one of 7 feet; that is, when working at a mean rate, the 30 feet ſail will be equal to the power of 18,3 men, or of 3⅔ horſes; reckoning 5 men to a horſe: whereas the effect of the common Dutch ſails, of the ſame length, being leſs in the proportion of 820 : 442, will be ſcarce equal to the power of 10 men, or of two horſes.

That theſe computations are not merely ſpeculative, but will nearly hold good when applied to works in large, I have had an opportunity of verifying: for in a mill with the enlarged ſails of 30 feet, applied to the cruſhing of rape ſeed, by means of two runners upon the edge, for making oil; I obſerved, that when the ſails made 11 turns in a minute, in which caſe the velocity of the wind was about 13 feet in a ſecond, according to article 6th, the runners then made 7 turns in a minute: whereas 2 horſes, applied to the ſame 2 runners, ſcarcely worked them at the rate of 3½ turns in the ſame time. Laſtly, with regard to the real ſuperiority of the enlarged ſails, above the Dutch ſails as commonly made, it has ſufficiently appeared, not only in thoſe caſes where they have been applied to new mills, but where they have been ſubſtituted in the place of the others.

VIII. Concerning horizontal Windmills and Water-wheels, with oblique Vanes.

Obſervations upon the effects of common windmills, with oblique vanes, have led many to imagine, that could the vanes be brought to receive the direct impulſe, like a ſhip ſailing before the wind, it would be a very great improvement in point of power: while others attending to the extraordinary and even unexpected effects of oblique

oblique vanes have been led to imagine that oblique vanes applied to water-mills, would as much exceed the common water-wheels, as the vertical wind-mills are found to have exceeded all attempts towards an horizontal one. Both theſe notions, but eſpecially the firſt, have ſo plauſible an appearance, that of late years there has ſeldom been wanting thoſe, who have aſſiduouſly employed themſelves to bring to bear deſigns of this kind: it may not therefore be unacceptable to endeavour to ſet this matter in a clear light.

PLATE VI. fig. 2. Let A B be the ſection of a plane, upon which let the wind blow in the direction C D, with ſuch a velocity as to deſcribe a given ſpace B E, in a given time (ſuppoſe 1 ſecond); and let A B be moved parallel to itſelf, in the direction C D. Now, if the plane A B moves with the ſame velocity as the wind; that is, if the point B moves through the ſpace B E in the ſame time that a particle of air would move through the ſame ſpace; it is plain that, in this caſe, there can be no preſſure or impulſe of the wind upon the plane: but if the plane moves ſlower than the wind, in the ſame direction, ſo that the point B may move to F, while a particle of air, ſetting out from B at the ſame inſtant, would move to E, then B F will expreſs the velocity of the plane; and the relative velocity of the wind and plane will be expreſſed by the line F E. Let the ratio of F E to B E be given (ſuppoſe 2 : 3); let the line A B repreſent the impulſe of the wind upon the plane A B, when acting with its whole velocity B E; but, when acting with its relative velocity F E, let its impulſe be denoted by ſome aliquot part of A B, as for inſtance $\frac{4}{9}$ A B: then will $\frac{4}{9}$ of the parallelogram A F repreſent the mechanical power of the plane; that is, $\frac{4}{9}$ A B × $\frac{1}{3}$ B E.

2dly, Let I N be the ſection of a plane, inclined in ſuch a manner, that the baſe I K of the rectangle triangle I K N may be equal to A B; and the perpendicular N K = B E; let the plane I N be ſtruck by the wind, in the direction L M, perpendicular to I K: then, according to the known rule of oblique forces, the impulſe of the wind upon the plane I N, tending to move it according to the direction L M, or N K, will be denoted by the baſe I K; and that part of the impulſe, tending to move it according to the direction I K, will be expreſſed by the perpendicular N K. Let the plane I N be moveable in the direction of I K only; that is, the point I in the direction of I K, and the point N in the direction N Q, parallel thereto. Now it is evident, that if the point I moves through the line I K, while a particle of air, ſetting forwards at the ſame time from the point N, moves through the line N K, they will both arrive at the point K at the ſame time; and conſequently, in this caſe alſo, there can be no preſſure or impulſe of the particle of the air upon the plane I N. Now let I O be to I K as B F to B E; and let the

the plane I N move at ſuch a rate, that the point I may arrive at O, and acquire the poſition I Q; in the ſame time that a particle of wind would move through the ſpace N K: as O Q is parallel to I N; (by the properties of ſimilar triangles) it will cut N K in the point P, in ſuch a manner, that N P = B F, and P K = F E: hence it appears, that the plane I N, by acquiring the poſition O Q, withdraws itſelf from the action of the wind, by the ſame ſpace N P, that the plane A B does by acquiring the poſition F G; and conſequently, from the equality of P K to F E, the relative impulſe of the wind P K, upon the plane O Q, will be equal to the relative impulſe of the wind F E, upon the plane F G: and ſince the impulſe of the wind upon A B, with the relative velocity F E, in the direction B E, is repreſented by $\frac{4}{9}$ A B; the relative impulſe of the wind upon the plane I N, in the direction N K, will in like manner be repreſented by $\frac{4}{9}$ I K; and the impulſe of the wind upon the plane I N, with the relative velocity P K, in the direction I K, will be repreſented by $\frac{4}{9}$ N K: and conſequently the mechanical power of the plane I N, in the direction I K, will be $\frac{4}{9}$ the parallelogram I Q: that is $\frac{1}{3}$ I K × $\frac{4}{9}$ N K: that is, from the equality of I K = A B and N K = B E, we ſhall have $\frac{4}{9}$ I Q = $\frac{1}{3}$ A B × $\frac{4}{9}$ B E = $\frac{4}{9}$ A B × $\frac{1}{3}$ B E = $\frac{4}{9}$ of the area of the parallelogram A F. Hence we deduce this

GENERAL PROPOSITION.

That all Planes, however ſituated, that intercept the ſame Section of the Wind, and having the ſame relative Velocity, in regard to the Wind, when reduced into the ſame Direction, have equal Powers to produce mechanical Effects.

For what is loſt by the obliquity of the impulſe, is gained by the velocity of the motion.

Hence it appears, that an oblique ſail is under no diſadvantage in reſpect of power, compared with a direct one; except what ariſes from a diminution of its breadth, in reſpect to the ſection of the wind: the breadth I N being obliquity reduced to I K.

The diſadvantage of horizontal windmills therefore does not conſiſt in this; that each ſail, when directly expoſed to the wind, is capable of a leſs power, than an oblique one of the ſame dimenſions; but that in an horizontal windmill, little more than one ſail can be acting at once: whereas in the common windmill, all the four act together: and therefore,

therefore, ſuppoſing each vane of an horizontal windmill, of the ſame dimenſions as each vane of the vertical, it is manifeſt the power of a vertical mill with four ſails, will be four times greater than the power of the horizontal one, let its number of vanes be what it will; this diſadvantage ariſes from the nature of the thing: but if we conſider the further diſadvantage, that ariſes from the difficulty of getting the ſails back again againſt the wind, &c. we need not wonder if this kind of mill is in reality found to have not above $\frac{1}{7}$ or $\frac{1}{7\frac{1}{2}}$ of the power of the common ſort; as has appeared in ſome attempts of this kind.

In like manner, as little improvement is to be expected from water-mills with oblique vanes: for the power of the ſame ſection of a ſtream of water, is not greater when acting upon an oblique vane, than when acting upon a direct one: and any advantage that can be made by intercepting a greater ſection, which ſometimes may be done in the caſe of an open river, will be counterbalanced by the ſuperior reſiſtance, that ſuch vanes would meet with by moving at right angles to the current: whereas the common floats always move with the water nearly in the ſame direction.

Here it may reaſonably be aſked, that ſince our geometrical demonſtration is general, and proves, that one angle of obliquity is as good as another; why in our experiments it appears, that there is a certain angle which is to be preferred to all the reſt? It is to be obſerved, that if the breadth of the ſail I N is given, the greater the angle K I N, and the leſs will be the baſe I K: that is, the ſection of wind interſected, will be leſs: on the other hand, the more acute the angle K I N, the leſs will be the perpendicular K N: that is, the impulſe of the wind, in the direction I K being leſs, and the velocity of the ſail greater; the reſiſtance of the medium will be greater alſo. Hence therefore, as there is a diminution of the ſection of the wind intercepted on one hand, and an increaſe of reſiſtance on the other, there is ſome angle, where the diſadvantage ariſing from theſe cauſes upon the whole is the leaſt of all; but as the diſadvantage ariſing from reſiſtance is more of a phyſical than geometrical conſideration, the true angle will beſt be aſſigned by experiments.

Scholium.

In trying the experiments contained in Table III. and IV. the different ſpecific gravity of the air, which is undoubtedly different at different times, will cauſe a difference in the load, proportional to the difference of its ſpecific gravity, though its velocity

velocity remains the ſame; and a variation of ſpecific gravity may ariſe not only from a variation of the weight of the whole column, but alſo by the difference of heat of the air concerned in the experiment, and poſſibly of other cauſes; yet the irregularities that might ariſe from a difference of ſpecific gravity were thought to be too ſmall to be perceivable, till after the principal experiments were made, and their effects compared; from which, as well as ſucceeding experiments, thoſe variations were found to be capable of producing a ſenſible, though no very conſiderable effect: however, as all the experiments were tried in the ſummer ſeaſon, in the day time, and under cover, we may ſuppoſe that the principal ſource of error would ariſe from the different weight of the column of the atmoſphere at different times: but as this ſeldom varies above $\frac{1}{11}$ part of the whole, we may conclude, that though many of the irregularities contained in the experiments referred to in the foregoing eſſay, might ariſe from this cauſe; yet as all the principal concluſions are drawn from the medium of a conſiderable number, many whereof were made at different times, it is preſumed that they will nearly agree with the truth, and be altogether ſufficient for regulating the practical conſtruction of thoſe kind of machines, for which uſe they were principally intended.

AN

AN experimental Examination of the Quantity and Proportion of Mechanic Power necessary to be employed in giving different Degrees of Velocity to Heavy Bodies from a State of rest, by Mr. JOHN SMEATON, F. R. S.

Read before the Royal Society, April 25th, 1776.

ABOUT the year 1686, Sir ISAAC NEWTON firſt publiſhed his *Principia*, and, conformably to the language of mathematicians of thoſe times defined, that "the "quantity of motion is the meaſure of the ſame, ariſing from the velocity and quantity "of matter conjointly." Very ſoon after this publication, the truth or propriety of this definition was diſputed by certain philoſophers, who contended, that the meaſure of the quantity of motion ſhould be eſtimated by taking the quantity of matter and the ſquare of the velocity conjointly. There is nothing more certain, than that from equal impelling powers, acting for equal intervals of time, equal increaſes of velocity are acquired by given bodies, when unreſiſted by a medium; thus gravity cauſes a body, in obeying its impulſe during one ſecond of time, to acquire a velocity which would carry it uniformly forward, without any additional impulſe, at the rate of 32 ft. 2 in. *per* ſecond; and if gravity is ſuffered to act upon it for two ſeconds, it will have, in that time, acquired a velocity that would carry it, at an uniform rate, juſt double of the former; that is, at the rate of 64 ft. 4 in. *per* ſecond. Now, if in conſequence of this equal increaſe of velocity, in an equal increaſe of time, by the continuance of the ſame impelling power, we define that to be a double quantity of motion, which is generated in a given quantity of matter, by the action of the ſame impelling power for a double time; this will be co-incident with Sir ISAAC NEWTON's definition above mentioned; whereas, in trying experiments upon the total effects of bodies in motion, it appears, that when a body is put in motion, by whatever cauſe, the impreſſion it will make upon an uniformly reſiſting medium, or upon uniformly yielding ſubſtances, will be as the maſs of matter of the moving body, multiplied by the ſquare of its velocity: the queſtion, therefore, properly is, whether thoſe terms, the *quantity of motion*, the *momenta* of bodies in motion, or *forces* of bodies in motion, which have generally been eſteemed ſynonymous, are with the moſt propriety of language to be eſteemed equal, double, or triple, when they have been generated by an equable impulſe, acting for an equal, double, or triple, time; or that it ſhould be meaſured by the effects being equal, double, or triple, in overcoming reſiſtances before a body in motion can be ſtopped?

ſtopped? For, according as thoſe terms are underſtood in this or that way, it will neceſſarily follow, that the *momenta* of equal bodies will be as the velocities, or as the ſquares of the velocities, or as the ſquares of the velocities reſpectively; it being certain, that, whichever we take for the proper definition of the term quantity of motion, by paying a proper regard to the collateral circumſtances that attend the application of it, the ſame concluſion, in point of computation, will reſult. I ſhould not, therefore, have thought it worth while to trouble the Society upon this ſubject, had I not found, that not only myſelf and other practical artiſts, but alſo ſome of the moſt approved writers, had been liable to fall into errors, in applying theſe doctrines to practical mechanics, by ſometimes forgetting or neglecting the due regard which ought to be had to theſe collateral circumſtances. Some of theſe errors are not only very conſiderable in themſelves, but alſo of great conſequence to the public, as they tend greatly to miſlead the practical artiſt in works that occur daily, and which often require very great ſums of money in their execution. I ſhall mention the following inſtances.

DESAGULIERS, in his ſecond volume of Experimental Philoſophy, treating upon the queſtion concerning the forces of bodies in motion, after taking much pains to ſhew that the diſpute, which had then ſubſiſted fifty years, was a diſpute about the meaning of words; and that the ſame concluſion will be brought out, when things are rightly underſtood, either upon the old or new opinion, as he diſtinguiſhes them; among other things, tells us, that the old and new opinion may be eaſily reconciled in this inſtance: that the wheel of an underſhot water-mill is capable of doing quadruple work when the velocity of the water is doubled, inſtead of double work only; "becauſe (the adjutage "being the ſame), ſays he, we find, that as the water's volocity is double, there are "twice the number of particles of water that iſſue out, and therefore the ladle-board is "ſtruck by twice the matter, which matter moving with twice the velocity that it had "in the firſt caſe, the whole effect muſt be quadruple, though the inſtantaneous ſtroke "of each particle is increaſed only in a ſimple proportion of the velocity." See vol. II. Annotations on lecture 6th, p. 92.

Again, in the ſame volume, lecture 12th, p. 424, referring to what went before, he tells us, "The knowledge of the foregoing particulars is abſolutely neceſſary for ſetting "an underſhot wheel to work: but the advantage to be reaped from it would be ſtill "gueſswork, and we ſhould be ſtill at a loſs to find out the utmoſt it can perform, if "we had not an ingenious propoſition of that excellent mechanic M. PARENT, of the "Royal Academy of Sciences, who has given us a *maximum* in this caſe, by ſhewing,

" that an undershot wheel can do the most work, when its velocity is equal to the third " part of the velocity of the water that drives it, &c. because then two-thirds of the " water is employed in driving the wheel with a force proportionable to the square of its " velocity. If we multiply the surface of the adjutage or opening by the height of the " water, we shall have the column of water that moves the wheel. The wheel thus " moved will sustain on the opposite side only four-ninths of that weight, which will " keep it in equilibrio; but what it can move with the velocity it goes with, will be " but one-third of that weight of equilibrium; that is, $\frac{4}{27}$ths of the weight of the first " column, &c.—This is the utmost that can be expected."

The same conclusion is likewise adopted by MACLAURIN, in art. 907. p. 728. of his Fluxions, where, giving the fluxionary deduction of M. PARENT's proposition, he says, " that if A represents the weight which would balance the force of the stream, when its " velocity is a; and v represents the velocity of the part of the engine, which it strikes " when the motion of the machine is uniform, &c.—the machine will have the greatest " effect when v is equal to $\frac{a}{3}$; that is, if the weight that is raised by the engine be less " than the weight which would balance the power, in the proportion of 4 to 9, and the " *momentum* of the weight is $\frac{4Aa''}{27}$".

Finding that these conclusions were far from the truth; and seeing, from many other circumstances, that the practical theory of making water and wind-mills was but very imperfectly delivered by any author I had then an opportunity of consulting *; in the

* BELIDOR, *Architecture Hydraulique*, greatly prefers the application of water to an undershot mill, instead of an overshot; and attempts to demonstrate, that water applied undershot will do six times more execution than the same applied overshot. See vol. I. p. 266. While DESAGULIERS, endeavouring to invalidate what had been advanced by BELIDOR, and greatly preferring an overshot to an undershot, says, Annotat. on lecture 12, vol. II. p. 532, that from his own experience, " a well-made overshot mill " ground as much corn in the same time with ten times less water;" so that betwixt BELIDOR and DESAGULIERS, here is a difference of no less than 60 to 1.

Again, BELIDOR, vol. II. p. 72, says, that the centre of gravity of each sail of a wind-mill should travel in its own circle with one-third of the velocity of the wind; so that, taking the distance of this centre of gravity from the centre of motion at 20 feet, as he states it, p. 38, article 849, the circumference will be exceeding 126 feet English measure: a wind, therefore, to make the mill go twenty turns *per* minute, which they frequently do with a fresh wind and all their cloth spread, would require the wind to move above 80 miles an hour; a velocity perhaps hardly equalled in the greatest storms we experience in this climate.

year

year 1751 I began a courſe of experiments upon this ſubject. Theſe experiments, with the concluſions drawn from them, have already been communicated to this Society, who printed them in vol. LI. of their Tranſactions for the year 1759, and for this communication I had the honour of receiving the annual medal of Sir GODFREY COPLEY, from the hands of our very worthy Preſident the late Earl of MACCLESFIELD. Thoſe experiments and concluſions ſtand uncontroverted, ſo far as I know, to this day; and having ſince that time been concerned in directing the conſtruction of a great number of mills, which were all executed upon the principles deduced from them, I have by that means had many opportunities of comparing the effects actually produced with the effects which might be expected from the calculation; and the agreement, I have always found between theſe two, appears to me fully to eſtabliſh the truth of the principles upon which they were conſtructed, when applied to great works, as well as upon a ſmaller ſcale in models.

Reſpecting the explanatory deduction of DESAGULIERS in the firſt example above-mentioned, which, indeed, I have found to be the commonly received doctrine among theoretical mechanics, it is ſhewn above, in my former Eſſay, page 41, &c. part 1. maxim 4. that, where the velocity of water is double, the adjutage or aperture of the ſluice remaining the ſame, the effect is eight times; that is, not as the ſquare but as the cube of the velocity; and the ſame is inveſtigated concerning the power of the wind ariſing from difference of velocity, p. 67. being part 3. maxim 4.

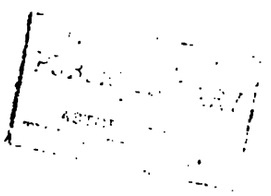
The concluſion in the ſecond example above-mentioned, adopted both by DESAGULIERS and MACLAURIN, is not leſs wide of the truth than the foregoing; for if that concluſion were true, only $\frac{4}{27}$ths of the water expended could be raiſed back again to the height of the reſervoir from which it had deſcended, excluſively of all kinds of friction, &c. which would make the actual quantity raiſed back again ſtill leſs; that is, leſs than one-ſeventh of the whole; whereas it appears, from Table I. of the preceding eſſay, that in ſome of the experiments there related, even upon the ſmall ſcale on which they were tried, the work done was equivalent to the raiſing back again about one quarter of the water expended; and in large works the effect is ſtill greater, approaching towards half, which ſeems to be the limit for the underſhot mills, as the whole would be the limit for the overſhot mills, if it were poſſible to ſet aſide all friction, reſiſtance from the air, &c.

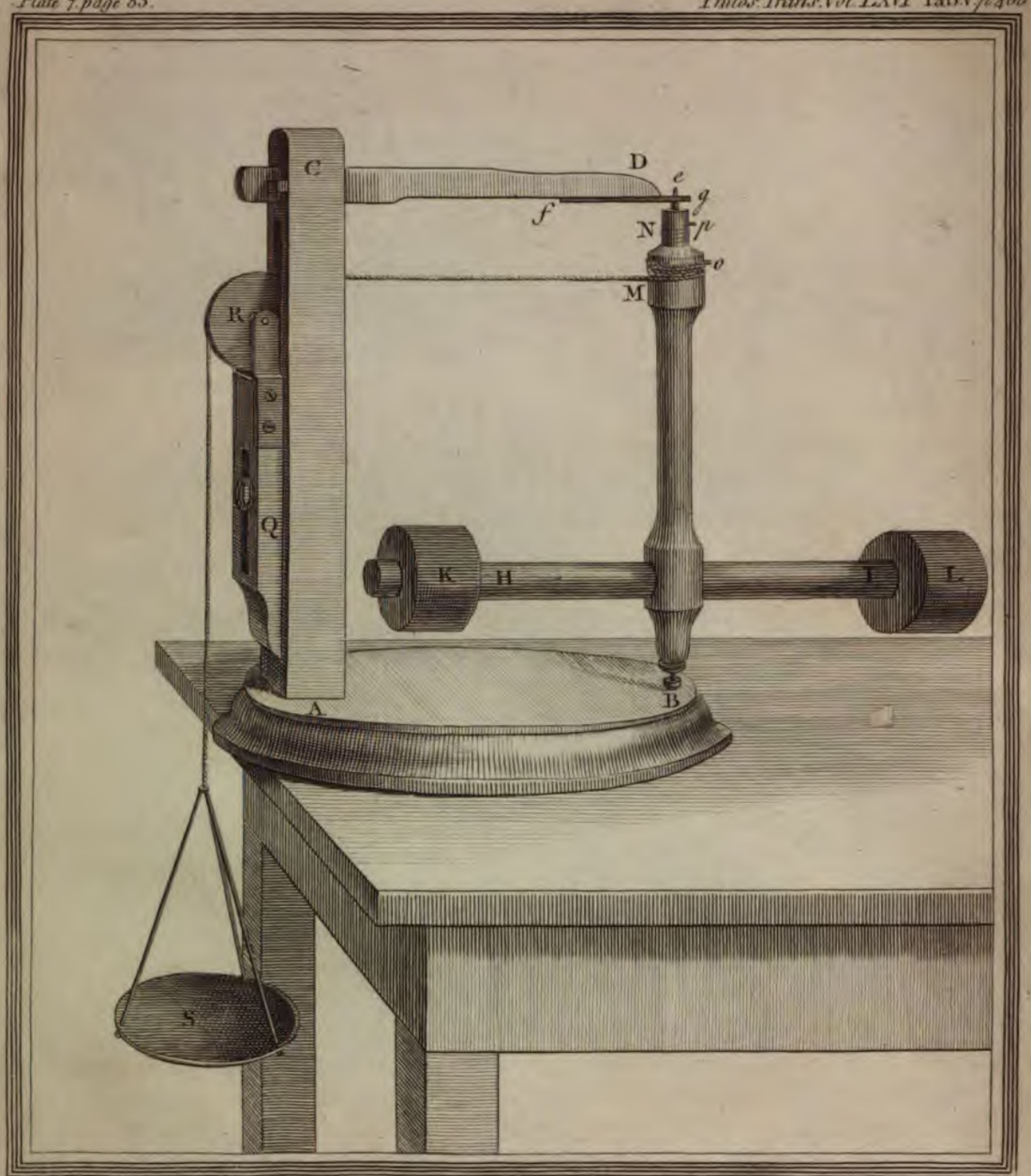
The velocity alſo of the wheel, which, according to M. PARENT's determination, adopted by DESAGULIERS and MACLAURIN, ought to be no more than one-third of that of

of the water, varies at the *maximum* in the abovementioned experiments of Table I. between one-third and one-half; but in all the caſes there related, in which the moſt work is performed in proportion to the water expended, and which approach the neareſt to the circumſtances of great works, when properly executed, the *maximum* lies much nearer to one-half than one-third; one-half, ſeeming to be the true *maximum*, if nothing were loſt by the reſiſtance of the air, the ſcattering of the water carried up by the wheel, and thrown off by the centrifugal force, &c. all which tend to diminiſh the effect more, at what would be the *maximum* if theſe did not take place, than they do when the motion is a little ſlower.

Finding theſe matters, as well as others, to come out in the experiments, ſo very different from the opinions and calculations of authors of the firſt reputation, who, reaſoning according to the Newtonian definition, muſt have been led into theſe errors from a want of attending to the proper collateral circumſtances; I thought it very material, eſpecially for the practical artiſt, that he ſhould make uſe of a kind of reaſoning in which he ſhould not be ſo liable to miſtakes; in order, therefore, to make this matter perfectly clear to myſelf, and poſſibly ſo to others, I reſolved to try a ſet of experiments from whence it might be inferred, what proportion or quantity of mechanical power is expended in giving the ſame body different degrees of velocity. This ſcheme was put in execution in the year 1759, and the experiments were then ſhewn to ſeveral friends, particularly my very worthy and ingenious friend Mr. WILLIAM RUSSELL.

In my experimental inquiry concerning the powers of water and wind before referred to, I have, p. 30, part 1, defined what I meant by power, as applied to practical mechanics, that is, what I now call mechanical power; which, in terms ſynonymous to thoſe there uſed, may be ſaid to be meaſured by multiplying the weight of the body into the perpendicular height from which it can deſcend; thus the ſame weight, deſcending from a double height, is capable of producing a double mechanical effect, and is therefore a double mechanical power. A double weight deſcending from the ſame height is alſo a double power, becauſe it likewiſe is capable of producing a double effect; and a given body, deſcending through a given perpendicular height, is the ſame power as a double body deſcending through half that perpendicular; for, by the intervention of proper levers, they will counter-balance one another, conformably to the known laws of mechanics, which have never been controverted. It muſt, however, be always underſtood, that the deſcending body, when acting as a meaſure of power,

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power, is ſuppoſed to deſcend ſlowly, like the weight of a clock or a jack; for, if quickly deſcending, it is ſenſibly compounded with another law, viz. the law of acceleration by gravity.

DESCRIPTION OF THE MACHINE.

PLATE VI.

A B is the baſe of the machine placed upon a table.

A C is a pillar or ſtandard.

C D is an arm, upon the extremity of which is fixed a plate *f g*, which is here ſeen edge-ways, through which is a ſmall hole for receiving a ſmall ſteel pivot *e*, fixed in the top of the upright axis *e* B; the lower end of this axis finiſhes in a conical ſteel point, which reſts upon a ſmall cup of hard ſteel poliſhed at B.

H I is a cylinder of white fir, which paſſes through a perforation in the axis, and therein fixes; and, upon the two arms formed thereby, are capable of ſliding.

K L two cylindric weights of lead of equal ſize, which are capable of being fixed upon any part of the cylindric arms, from the axis to their extremities, by means of two thin wedges of wood. The two weights, therefore, being at equal diſtances from the centre, and the axis perpendicular, the whole will be balanced upon the point at B, and moveable thereupon by an impelling power with very little friction.

Upon the upper part of the axis are formed M N, two cylindrical barrels, whereof M is double the diameter of N: they have a little pin ſtuck into one ſide of each at *o*, *p*.

Q is a piece capable of ſliding higher or lower, as occaſion requires; and carries

R, a light pulley of about three inches diameter, hung upon a ſteel axis, and moveable upon two ſmall pivots. The plane of the pulley, however, is not directed to the middle of the upright axis, but a little on one ſide, ſo as to point (at a mean) between the ſurface of the bigger barrel and the leſs.

s is a light ſcale for receiving weights, and hangs by a ſmall twine, cord, or line, that paſſes the pulley and terminates either upon the bigger barrel or the leſs, as may be required; the ſliding-piece q being moved higher or lower for each, that the line, in paſſing from the pulley to the barrel, may be nearly horizontal. The end of the line that is furtheſt from the ſcale is terminated by a ſmall loop, which hangs on upon the pin *o*, or the pin *p*, according as the bigger or the leſſer barrel is to be uſed.

Now, having wound up a certain number of turns of the line upon the barrel, and having placed a weight in the ſcale s, it is obvious, that it will cauſe the axis to turn round, and give motion to its arms, and to the weights of lead placed thereon, which are the heavy bodies to be put in motion by the impulſe of the weight in the ſcale; and when the line is wound off to the pin, the loop ſlips off, and the ſcale then falling down, the weight will ceaſe to accelerate the motion of the heavy bodies, and leave them revolving, equably forward, with the velocity they have acquired, except ſo far as it muſt be gradually leſſened by the friction of the machine and reſiſtance of the air, which being ſmall, the bodies will revolve ſometime before their velocity is apparently diminiſhed.

Meaſures of ſome Parts of the Machine.

	Inches.
Diameter of the cylinders of lead, or the heavy bodies, -	2,57
Length of ditto, - - - - - - - - -	1,56
Diameter of the hole therein, - - - - -	,72

Weight of each cylinder 3 lbs. Avoirdupois.

Greater diſtance of the middle of each body from the centre of the axis, - - - - - - -	8,25
The ſmaller diſtance of ditto, - - - - -	3,92
10 turns of the ſmaller barrel raiſes the ſcale, 5 ditto of the bigger ditto, - - - - - - -	25,25

When the bodies are at the ſmaller diſtance above ſpecified from the axis of rotation, they are then in effect at half the greater diſtance from that axis; for, ſince the axis itſelf,

itſelf, and the cylindric arms of wood, keep an unvaried diſtance from the centre of rotation, the bodies themſelves muſt be moved nearer than half their former diſtance, in order that, compounded with the unvariable parts, they may be virtually at the half diſtance. In order to find this half diſtance nearly, I put in an arm of the ſame wood, that only went through the axis, without extending in the oppoſite direction; one of the bodies being put upon the end of this arm, at the diſtance of 8,25 inches, the whole machine was inclined till the body and arm became a kind of pendulum, and vibrated at the rate of 92 times *per* minute; and as a pendulum of the half length vibrates quicker in the proportion of $\surd 1$ to $\surd 2$; that is, in the proportion of 92 to 130 nearly; therefore, keeping the ſame inclination of the machine, the weight was moved upon the arm till it made 130 vibrations *per* minute; which was found to be, when it was at 3,92 inches diſtance from the centre as above ſtated, which is about $\frac{2}{10}$ths of an inch nearer than the half diſtance. The double arm was then put in, and marked accordingly, and the bodies being mounted thereon, the whole was adjuſted ready for uſe; and with it were tried the following experiments, each of which was repeated ſo many times as to be fully ſatisfactory.

Table of Experiments.

Number.	Ounces Avoirdupois in the Scale.	Barrel uſed, M the bigger, N the ſmaller.	The Arms, W the whole, H the half-length.	Number of Turns of the Line wound on the Barrel.	Time of the Deſcent of the Weight in the Scale.	Time in making 20 Revolutions with equal Motions.
					″	′
1	8	M	W	5	14¼	29
2	8	N	W	10	28½	29½
3	8	N	W	2½	14¼	58½
4	32	M	W	5	7	14
5	32	N	W	10	14	14¾
6	32	N	W	2½	7	28¾
7	8	M	H	5	7	14¾
8	8	N	H	10	14	15
9	8	N	H	2½	7	30¼
1	2	3	4	5	6	7

The

The $58''\frac{1}{2}$ in number 3, column 7, was determined in fact from $29'\frac{1}{4}$, being the time of making 10 equable revolutions after the weight was dropped off, in order to prevent the ſenſible retardation that might take place, and affect the obſervation, if continued for 20 revolutions made ſo ſlowly.

Further Definitions.

I have already defined what I mean by mechanic power; but, before I proceed further, it will be neceſſary alſo to define the following terms:

Impulſe or Impulſion,
Impulſive Force or Power,
Impelling Force or Power,
} By all which, I underſtand the uniform endeavour that one body exerts upon another, in order to make it move; and that, whether it produces or generates motion by this endeavour or not; and the quantity of this impelling power may be meaſured either by its being a weight of itſelf, or by being counterbalanced by a weight. It may alſo act either immediately upon the body to be moved, ſo that if motion is the conſequence, they move with the ſame velocity; and that, either by a ſimple contact, or by being drawn as by a cord, or puſhed as by a ſtaff: or it may act by the intervention of a lever or other mechanic inſtrument, in which the velocity of the body to be moved may be very different from the velocity of the impelling power or mover; but in comparing them, the impelling powers muſt be reduced according to the proportional velocities of the mover and moved; or, in levers of different lengths, they may be compared by a ſtandard length of lever, which is the method taken in the ſubſequent reaſoning upon the preceding experiments. An impelling power, therefore, conſiſting of a double weight, or requiring a double weight to counter-balance it, when acting with equal levers, is a double impelling power, or an impelling power of double the intenſity.

Obſervations and Deductions from the preceding Experiments.

1ſt, By the firſt experiment it appears, that the mechanic power employed, conſiſting of 8 ounces in the ſcale, deliberately deſcending (by 5 turns of the bigger barrel) through a perpendicular ſpace $25\frac{1}{4}$ inches, will repreſent the quantity of mechanic power which cauſes the two heavy bodies, from a ſtate of reſt, to acquire a velocity, ſuch as to carry them equably through 20 circumferences of their circle of revolution in the ſpace of $29''$; and that the time in which the mechanic power produced this effect was $14''\frac{1}{4}$, as appears by column 6th. And this mechanic power we ſhall expreſs by the number

number 202, the product of the number of ounces in the ſcale multiplied by the inches in its perpendicular deſcent, for $8 \times 25\frac{1}{4} = 202$.

2dly, By the ſecond experiment, as 10 turns of the ſmaller barrel are equal to the ſame perpendicular height as 5 turns of the bigger, it follows, that the ſame mechanic power, viz. 202, acting upon the ſame heavy bodies to accelerate them, produces the very ſame effect in generating motion in the bodies as it did before, viz. 20 revolutions in $29''\frac{1}{4}$, the ſmall difference of $\frac{1}{4}$ of a ſecond being no more than may reaſonably be attributed to the unavoidable errors ariſing from friction of the machine, want of perfect accuracy in its meaſures, reſiſtance of the air, and imperfections in the obſervations themſelves, which muſt not only be allowed for in this, but the reſt; but as the impelling power is acting here upon a lever of but half the length, and, conſequently, but half the intenſity, when referred to the bodies to be moved, it takes juſt double the time to generate the ſame velocity therein.

Deduction. It appears from hence, that the ſame mechanic power is capable of producing the ſame velocity in a given body, whether it is applied ſo as to produce it in a greater or leſſer time; but that the time taken to produce a given velocity, by an uniformly continued action, is in a ſimple inverſe proportion of the intenſity of the impulſive power.

3dly, The third experiment being made with $2\frac{1}{2}$ turns of the leſſer barrel, the ſame weight in the ſcale of 8 ounces deſcending only one quarter part of the former perpendicular, the mechanic power employed will be only one quarter part of the former, viz. $50\frac{1}{2}$; but as one quarter part of the mechanic power produces half of the former velocity in the heavy bodies; that is, they make 20 revolutions in $58''\frac{1}{2}$; that is, nearly 10 revolutions in $29''$; we may conclude, in this inſtance, that the mechanic power, employed in producing motion, is as the ſquare of the velocity produced in the ſame body; and that the velocity produced is as the time that an impelling power, of the ſame intenſity, continues to act upon it, as appears by the near agreement of numbers 2 and 3, column 6th.

4thly, In the fourth experiment, the apparatus is the ſame as the firſt, only here the weight in the ſcale is 32 ounces; that is, the impelling power is the quadruple of the firſt, and hereby a double velocity is given to the bodies; for they make 20 revolutions in $14''$, which is a ſmall matter leſs than half the time taken up in making 20 revolutions in the firſt experiment. It alſo appears, that the velocity acquired is

ſimply as the impelling power compounded with the time of its action: for a quadruple impulſion acting for 7″ inſtead of 14″ generates a double velocity, while the mechanic power employed to generate it is quadruple, for $32\times25\frac{1}{4}=808$. And here the mechanic power employed being four times greater than the firſt, it holds here alſo, that the mechanic power, to be neceſſarily employed, is as the ſquare of the velocity to be generated; that is, in the ſame proportion as turned out in the third experiment, where the mechanic power employed was only a quarter part of the firſt.

5thly, The fifth and ſixth experiments were made with a mechanic power four times greater than thoſe employed in numbers 2 and 3 reſpectively; and ſince the ſame deductions reſult from hence as from numbers 2 and 3, they are additional confirmations of the concluſions drawn from them and from the laſt article.

6thly, In the ſeventh experiment, the diſpoſition of the apparatus is the ſame as number 1, only here the bodies are placed upon the arms at the half-length; from whence it appears, that the ſame mechanic power ſtill produces the ſame velocity in the ſame bodies; for though 20 revolutions were performed in $14\frac{3}{4}$″ (ſee column 7) which is nearly half the time that 20 revolutions were performed in the firſt experiment; yet, ſince the circles in which the bodies revolved in the ſeventh are only of half the circumference as thoſe of number 1, it is obvious, that the abſolute velocity acquired by the moving bodies in the two caſes is equal. But, by column 6th, the time in which it was generated is only half; yet, notwithſtanding, this will coincide with the former concluſions, if the intenſity of the impelling power is compounded therewith; for, though the barrel was the ſame with the ſame number of turns as in number 1, and therefore the lever the ſame, by which the impelling power acted, yet, as the bodies, upon which this lever was to act, were placed upon a lever of only half the length from the centre, the impelling power acting by the firſt lever, would act upon the ſecond with double the intenſity, according to the known laws of mechanics; that is, it would require a double weight oppoſing the bodies, to prevent their moving, in order to balance it. An impulſive power, therefore, of double the intenſity, acting for half the time, produces the ſame effect in generating motion, as an impulſive power, of half the intenſity, acting for the whole time.

7thly, The eighth and ninth experiments afford the ſame deductions and confirmations relative to the ſeventh experiment, that the fifth and ſixth do reſpecting the fourth, and that the ſecond and third do reſpecting the firſt; and from the near agreement

ment of the whole, when the neceſſary allowances before mentioned are made, together with ſome ſmall inequality ariſing from the mechanical power loſt by the difference of the motion given by gravity to the weight in the ſcale: I ſay, from theſe agreements, under the very different mechanical powers applied, which were varied in the proportion of 1 to 16, we may ſafely conclude, that this is the univerſal law of nature, reſpecting the capacities of bodies in motion to produce mechanical effects, and the quantity of mechanic power neceſſary to be employed to produce or generate different velocities (the bodies being ſuppoſed equal in their quantity of matter); that the mechanic powers to be expended are as the ſquares of the velocities to be generated, and *vice verſâ*; and that the ſimple velocities generated are as the impelling power compounded with, or multiplied by, the time of its action, and *vice verſâ*.

We ſhall, perhaps, form a ſtill clearer conception of the relation between velocities produced, and the quantity of mechanic power required to produce them; together with the collateral circumſtances attending, by which theſe propoſitions, ſeemingly two, are reconciled and united, by ſtating the following popular elucidation, which indeed was the original idea that occurred to me on conſidering this ſubject; to put which to an experimental proof gave birth to the foregoing apparatus and experiments.

Suppoſe then a large iron ball of 10 feet diameter, turned truly ſpherical, and ſet upon an extended plane of the ſame metal, and truly level. Now, if a man begins to puſh at it, he will find it very reſiſting to motion at firſt; but, by continuing the impulſe, he will gradually get it into motion, and having nothing to reſiſt it but the air, he will, by continuing his efforts, at length get it to roll almoſt as faſt as he can run. Suppoſe now, in the firſt minute he gets it rolled through a ſpace of one yard; by this motion, proceeding from reſt (ſimilar to what happens to falling bodies) it would continue to roll forward at the rate of two yards *per* minute, without further help; but ſuppoſing him to continue his endeavours, at the end of another minute he will have given it a velocity capable of carrying it through a ſpace of two yards more, in addition to the former, that is, at the rate of four yards *per* minute; and at the end of the third minute, he has again added an equal increaſe of velocity, and made it proceed at the rate of ſix yards *per* minute; and ſo on, increaſing its velocity at the rate of two yards in every minute. The man, therefore, in the ſpace of every minute exerts an equal impulſe upon the ball, and generates an equal increaſe of movement correſpondent to the definition of Sir ISAAC NEWTON. But let us ſee what happens beſides: the man, in the firſt minute, has moved but one yard from where he ſet out; but he muſt in the

ſecond

ſecond minute move two yards more, in order to keep up with the ball; and as he exerted an impulſe upon it, ſo as at the end of the ſecond minute to have given it an additional velocity of the two yards, he muſt alſo in this time have gradually changed its velocity from the rate of two yards *per* minute to that of four, and the ſpace, that he will of conſequence have actually been obliged to go through in the ſecond minute, will be according to the mean of the extremes of velocity at the beginning and end thereof, that is, three yards in the ſecond minute; ſo that being one yard from his original place at the begining of the ſecond minute, at the end of it he will have moved the ſum of the journies of the firſt and ſecond minute, that is, in the whole four yards from his original place. As he has now generated a velocity in the ball of four yards *per* minute, the third minute he muſt travel four yards to keep up with the ball, and one more in generating the equal increment of velocity; ſo that in the third minute, he muſt travel five yards to keep up the ſame impelling power upon the ball that he did in the firſt minute in travelling one, ſo that theſe five yards in the third minute, added to the four yards that he had travelled in the two preceding minutes, ſets him at the end of the third minute nine yards from whence he ſet out, having then given the ball a velocity capable of carrying it uniformly forward at the rate of ſix yards *per* minute, as before ſtated. We may now leave the further purſuit of theſe proportions, and ſee how the account ſtands. He generated a velocity of two yards *per* minute in the firſt minute, the ſquare of which is four, when he had moved but one yard from his place; and he had generated a velocity of ſix yards *per* minute, the ſquare of which is thirty-ſix, at the end of the third minute, when he had travelled nine yards from his place. Now, ſince the ſquare of the velocity, generated at the end of the firſt minute, is to that of the velocity generated at the end of the third minute, as $4 : 36$, that is, as $1 : 9$; and ſince the ſpaces, moved through by the man to communicate theſe velocities, are alſo as $1 : 9$, it follows, that the ſpaces through which the man muſt travel, in order to generate theſe velocities reſpectively (preſerving the impelling power perfectly equal), muſt be as the ſquares of the velocities communicated to the ball; for, if the man was to be brought back again to his original place by a mechanical power, equally exerted upon the man equally reſiſting, this would be the meaſure of what the man has done in order to give a motion to the ball. It therefore directly follows, conformably to what has been deduced from the experiments, that the mechanic power that muſt of neceſſity be employed in giving different degrees of velocity to the ſame body, muſt be as the ſquare of that velocity; and if the converſe of this propoſition did not hold, viz. that if a body in motion, in being ſtopped, would not produce a mechanical effect equal or proportional to the ſquare of its velocity, or to the mechanical power employed in producing it, the effect would not correſpond with its producing cauſe.

Thus

Thus the conſequences of generating motion upon a level plane exactly correſpond with the generating of motion by gravity; viz. that though in two ſeconds of time the equal impulſive power of gravity gives twice the velocity to a body that it does in one ſecond, yet this collateral circumſtance attends it, that at the end of the double time, in conſequence of the velocity acquired in the firſt half, the body has fallen from where it ſet forward through four times the perpendicular; and therefore, though the velocity is only doubled, yet four times the mechanical power has been conſumed in producing it, as four times the mechanical power muſt be expended in bringing up the fallen body to its firſt place.

This then appers to be the foundation, not only of the diſputes that have ariſen, but of the miſtakes that have been made, in the application of the different definitions of quantity of motion; that while thoſe that have adhered to the definition of Sir ISAAC NEWTON, have complained of their adverſaries, in not conſidering the time in which effects are produced, they themſelves have not always taken into the account the ſpace that the impelling power is obliged to travel through, in producing the different degrees of velocity. It ſeems, therefore, that, without taking in the collateral circumſtances both of time and ſpace, the terms, quantity of motion, *momentum*, and force of bodies in motion, are abſolutely indefinite; and that they cannot be ſo eaſily, diſtinctly, and fundamentally compared, as by having recourſe to the common meaſure, viz. mechanic power.

From the whole of what has been inveſtigated, it therefore appears, that time, properly ſpeaking, has nothing to do with the production of mechanical effects, otherwiſe than as, by equally flowing, it becomes a common meaſure; ſo that, whatever mechanical effect is found to be produced in a given time, the uniform continuance of the action of the ſame mechanical power will, in a double time, produce two ſuch effects, or twice that effect. A mechanical power, therefore, properly ſpeaking, is meaſured by the whole of its mechanical effect produced, whether that effect is produced in a greater or a leſſer time; thus, having treaſured up 1000 tuns of water, which I can let out upon the overſhot wheel of a mill, and deſcending through a perpendicular of 20 feet, this power applied to proper mechanic inſtruments, will produce a certain effect, that is, it will grind a certain quantity of corn; and that at a certain rate of expending it, it will grind this corn in an hour. But ſuppoſe the mill equally adapted to produce a proportionable effect, by the application of a greater impulſive power as with a leſs; then, if I let out the water twice as faſt upon the wheel, it will grind the corn twice as faſt, and both the water will be expended and the corn ground in half an hour. Here the ſame mechanical effect is produced;

produced; viz. the grinding a given quantity of corn, by the ſame mechanical power, viz. 1000 tuns of water deſcending through a given perpendicular of 20 feet, and yet this effect is in one caſe produced in half the time of the other. What time, therefore, has to do in the buſineſs is this: let the rate of doing the buſineſs, or producing the effect, be what it will, if this rate is uniform, when I have found by experiment what is done in a given time, then, proceeding at the ſame rate, twice the effect will be produced in twice the time, on ſuppoſition that I have a ſupply of mechanic power to go on with. Thus 1000 tuns of water, deſcending through 20 feet of perpendicular, being, as has been ſhewn, a given mechanic power, let me expend it at what rate I will, if when this is expended, I muſt wait another hour before it be renewed, by the natural flow of a river, or otherwiſe, I can then only expend twelve ſuch quantities of power in 24 hours; but if, while I am expending 1000 tuns in one hour, the ſtream renews me the ſame quantity, then I can expend 24 ſuch quantities of power in 24 hours; that is, I can go on continually at that rate, and the product or effect will be in proportion to time, which is the common meaſure; but the quantity of mechanic power ariſing from the flow of the two rivers, compared by taking an equal portion of time, is double in the one to the other, though each has a mill, that, when going, will grind an equal quantity of corn in an hour.

NEW

NEW Fundamental Experiments upon the Collision of Bodies, by Mr. JOHN SMEATON, F. R. S.

Read before the Royal Society, April 18th, 1782.

IT is univerſally acknowledged, that the firſt ſimple principles of ſcience cannot be too critically examined, in order to their being firmly eſtabliſhed; more eſpecially thoſe which relate to the practical and operative parts of mechanics, upon which much of the active buſineſs of mankind depends. A ſentiment of this kind occaſioned the preceding tract upon *Mechanic Power*. What I have now to offer was intended as a ſupplement thereto, and the experiments were, in part, tried; but the completion thereof was deferred at the time of its firſt publication, partly from want of leiſure; partly to avoid too great a length of the paper itſelf; and partly to avoid the bringing forward too many points at once.

My preſent purpoſe is to ſhew, that the true doctrine of the *collision of bodies* hangs as it were upon the ſame hook, as the doctrine of the gradual generation of motion from reſt, conſidered in that paper: that is, that whether bodies are put into gradual motion, and uniformly accelerated from reſt to any given velocity; or are put in motion, in an inſtantaneous manner, when bodies of any kind ſtrike one another; the motion, or ſum of the motions produced, has the ſame relation to mechanic power therein defined, which is neceſſary to produce the motion deſired. To prove this, and at the ſame time to ſhew ſome capital miſtakes in principle, which have been *aſſumed* as indiſputable truths by men of great learning, is the reaſon of my now purſuing the ſame ſubject.

I do not mean to point out the particular miſtakes which have been made by particular men, as that would lead me into too great a length: I ſhall therefore content myſelf with obſerving, that the laws of colliſion, which have been inveſtigated by mathematical philoſophers, are principally of three kinds; viz. thoſe relating to bodies perfectly *elaſtic*; to bodies perfectly unelaſtic, and perfectly *ſoft*; and to bodies perfectly unelaſtic, and perfectly *hard*. To avoid prolixity, I ſhall conſider in each, only the ſimple caſe of two bodies which are equal in weight or quantity of matter ſtriking one another. Reſpecting thoſe which are perfectly elaſtic, it is univerſally agreed, that when two ſuch bodies ſtrike one another, no motion is loſt; but that in all caſes, what is loſt by

by one is acquired by the other: and hence, that if an elaſtic body in motion ſtrikes another at reſt, upon the ſtroke the former will be reduced to a ſtate of reſt, and the latter will fly off with an equal velocity.

In like manner, if a non-elaſtic *ſoft* body ſtrikes another at reſt, they neither of them remain at reſt, but proceed together from the point of colliſion with exactly one half of the velocity that the firſt had before the ſtroke; this is alſo univerſally allowed to be true, and is fully proved by every good experiment upon the ſubject.

Reſpecting the third ſpecies of body, that is, thoſe that are non-elaſtic and yet perfectly hard: the laws of motion relating to them, as laid down by one ſpecies of philoſophers, have been rejected by another; the latter alledging, that there are no ſuch bodies to be found in nature whereon to try the experiment; but thoſe who have laid down and aſſigned the doctrine that would attend the colliſion of bodies, of this kind (if they could be found) have univerſally agreed, that if a non-elaſtic *hard* body was to ſtrike another of the ſame kind at reſt, that, in the ſame manner as is agreed concerning non-elaſtic ſoft bodies, they neither of them would remain at reſt, but would in like manner proceed from the point of colliſion, with exactly one half of the velocity that the firſt had before the ſtroke: in ſhort, they lay it down as a rule attending all non-elaſtic bodies, whether hard or ſoft, that the velocity after the ſtroke will be the ſame in both, viz. *one half* of the velocity of the original ſtriking body.

Here is therefore the aſſumption of a principle, which in reality is proved by no experiment, nor by any fair deduction of reaſon that I know of, viz. that the velocity of non-elaſtic *hard* bodies after the ſtroke muſt be the ſame as that reſulting from the ſtroke of non-elaſtic *ſoft* bodies; and the queſtion now is, whether it be true or not?

Here it may be very properly aſked, what ill effects can reſult to practical men, if philoſophers ſhould reaſon wrong concerning the effects of what does not exiſt in nature, ſince the practical men can have no ſuch materials to work upon, or miſjudge of? But it is anſwered, that they who infer an equality of effects between the two ſorts, may from thence be miſled themſelves, and in conſequence miſlead practical men in their reaſonings and concluſions concerning the ſort with which they have abundant concern, to wit, the non-elaſtic *ſoft bodies*, of which water is one, which they have much to do with in their daily practice.

Previous to the trying my experiment on mills, I never had doubted the truth of the doctrine, that the ſame velocity reſulted from the ſtroke of both ſorts of non-elaſtic bodies; but the trial of thoſe experiments made me clearly ſee, at leaſt the inconcluſiveneſs, if not the falſity of that doctrine: becauſe I found a reſult which I did not expect to have ariſen from either ſort; and for the which, when it appeared from experiment, I could ſee a ſubſtantial reaſon why it ſhould take place in one ſort, and that it was impoſſible that it could take place in the other; for if it did, the bodies could not have been perfectly *hard*, which would be contrary to the hypotheſis: of this deduction I have given notice in my ſaid tract on mills, page 49. *The effect therefore of overshot wheels, &c.*

It may alſo be ſaid, that ſince we have no bodies perfectly elaſtic, or perfectly unelaſtic and *ſoft*, why ſhould we expect bodies perfectly unelaſtic and *hard?* Why may not the effects be ſuch as ſhould reſult from a ſuppoſition of their being *imperfectly elastic* joined with their being *imperfectly hard?* But here I muſt obſerve, that the ſuppoſition appears to be a contradiction in terms.

We have bodies which are ſo nearly perfectly elaſtic, that the laws may be very well deduced and confirmed by them; and the ſame obtains with reſpect to non-elaſtic *ſoft* bodies; but concerning bodies of a mixed nature, which are by far the greateſt number, ſo far as they are wanting in elaſticity, they *are ſoft* and *bruiſe, yield,* or *leave a mark* in colliſion; and ſo far as they are not perfectly ſoft they are elaſtic, and obſerve a mixture of the law relative to each; but imperfectly elaſtic bodies, imperfectly hard, come in reality under the *ſame deſcription* as the former mixed bodies: for ſo far as they are imperfectly hard they are ſoft, and either *bruiſe* and *yield,* or leave a mark in the ſtroke; and ſo far as they want perfect elaſticity, they are non-elaſtic; that is to ſay, they are bodies imperfectly elaſtic, and imperfectly ſoft; and in fact I have never yet ſeen any bodies but what come under this deſcription. It ſeems, therefore, that reſpecting the *hardneſs* of bodies they differ in degrees of it, in proportion as they have a greater degree of tenacity or coheſion; that is, are further removed from perfect ſoftneſs, at the ſame time that their elaſtic ſprings, ſo far as they reach, are very ſtiff; and hence we may (by the way) conclude, that the ſame mechanic power that is required to change the figure in a *ſmall degree* of thoſe bodies that have the popular appellation of *hard bodies,* would change it in a *great degree* in thoſe bodies that approach towards ſoftneſs, by having a ſmall degree of tenacity or coheſion. In the former kind we may rank the harder kinds of *caſt iron,* and in the latter, *ſoft tempered clay.*

While the philoſophical world was divided by the diſpute about the *old* and *new opinion*, as it was called, concerning the powers of bodies in motion, in proportion to their different velocities; thoſe who held the old opinion contending, that it was as the velocity *ſimply*, aſked thoſe of the new, how, upon their principles, they would get rid of the concluſions ariſing from the doctrine of unelaſtic perfectly hard bodies? They replied, they found no ſuch bodies in nature, and therefore did not concern themſelves about them. On the other hand, thoſe of the new opinion aſked thoſe of the old, how they would account for the caſe of non-elaſtic ſoft bodies, where, according to them, the whole motion loſt by the ſtriking body was retained in the two after the ſtroke (the two bodies moving together with the half velocity), though the two non-elaſtic bodies had been bruiſed and changed their figure by the ſtroke; for, if no motion was loſt, the change of figure muſt be an effect without a cauſe? To obviate this, thoſe of the old opinion ſeriouſly ſet about proving, that the bodies might change their figure, without any loſs of motion in either of the ſtriking bodies.

Neither of theſe anſwers have appeared to me ſatisfactory, eſpecially ſince my mill experiments: for, with reſpect to the firſt, it is no proper argument to urge the impoſſibility of finding the proper material for an experiment, in anſwer to a concluſion drawn from an abſtract idea. On the other hand, if it can be ſhewn, that the figure of a body can be changed, without a *power*, then, by the ſame law, we might be able to make a *forge hammer* work upon a maſs of ſoft iron, without any other power than that neceſſary to overcome the friction, reſiſtance, and original *vis inertiæ*, of the parts of the machine to be put in motion: for, as no progreſſive motion is given the maſs of iron by the hammer (it being ſupported by the anvil), no power can be expended that way; and if none is loſt to the hammer from changing the figure of the iron, which is the only effect produced, then the whole power muſt reſide in the hammer, and it would jump back again to the place from which it fell, juſt in the ſame manner as if it fell upon a body perfectly elaſtic, upon which, if it did fall, the caſe would really happen: the power, therefore, to work the hammer would be the ſame, whether it fell upon an elaſtic or non-elaſtic body; an idea ſo very contrary to all experience, and even apprehenſion, of both the philoſopher and vulgar artiſt, that I ſhall here leave it to its own condemnation.

As nothing, however, is ſo convincing to the mind as experiments obvious to the ſenſes, I was very deſirous of contriving an experiment in point, and as I ſaw no hopes of finding matter to make a *direct* experiment, I turned my mind towards an indirect one:

one: ſo circumſcribed, however, as to prove inconteſtably, that the reſult of the ſtroke of two non-elaſtic perfectly hard bodies could not be the ſame as would reſult from the colliſion of two ſoft ones; that is, if it can be *bonâ fide* proved, that *one half* of the original power is loſt in the ſtroke of ſoft bodies by the change of figure (as was very ſtrongly ſuggeſted by the mill experiments); then ſince no ſuch loſs can happen in the colliſion of bodies perfectly hard, the reſult and conſequence of ſuch a ſtroke muſt be *different*.

The conſequence of a ſtroke of bodies perfectly hard, but void of elaſticity, muſt doubtleſs be different from that of bodies perfectly *elastic:* for having no ſpring the body at reſt could not be driven off with the velocity of the ſtriking body, for that is the conſequence of the action of the ſpring or elaſtic parts between them, as will be ſhewn in the reſult of the experiments; the ſtriking body will therefore not be ſtopped, and as the motion it loſes muſt be communicated to the other, from the equality of action and re-action, they will both proceed together, with an equal velocity, as in the caſe of non-elaſtic ſoft bodies: the queſtion, therefore, that remains is, what that *velocity must be?* It muſt be greater than that of the non-elaſtic ſoft bodies, becauſe there is no mechanical power loſt in the ſtroke. It muſt be leſs than that of the ſtriking body, becauſe, if equal, inſtead of a *loss* of motion by the colliſion it will be doubled. If, therefore, non-elaſtic ſoft bodies loſe half their motion, or mechanical power, by change of figure in colliſion, and yet proceed together with half the velocity, and the non-elaſtic hard bodies can loſe *none* in any manner whatever; then, as they muſt move together, their velocity muſt be ſuch as to preſerve the equality of the mechanic power, *unimpaired*, after the ſtroke the ſame as it was before it.

For example, let the velocity of the ſtriking body before the ſtroke be 20, and its maſs or quantity of matter 8; then, according to the rule deduced from the experiments in the tract on *Mechanic Power* (ſee exp. 3d and 4th) that power will be expreſſed by $20 \times 20 = 400$, which $\times 8 = 3200$; and if half of it is loſt in the ſtroke, in the caſe of non-elaſtic ſoft bodies, it will be reduced to 1600; which $\div 16$ the double quantity of matter, will give 100 for the ſquare of their velocity; the ſquare root of which being 10, will be the velocity of the two non-elaſtic ſoft bodies after the ſtroke, being juſt one half of the original velocity, as it is conſtantly found to be. But in the non-elaſtic hard bodies, no power being loſt in the ſtroke, the mechanic power will remain after it, as before it, $= 3200$; this, in like manner, being divided by 16, the double quantity of matter, will give 200 for the ſquare of the velocity, the ſquare root

of

of which is 14.14, &c. for their velocity after the ſtroke, which is to 10, the velocity of the non-elaſtic ſoft bodies after the ſtroke, as the ſquare root of 2 to 1, or as the diagonal of a ſquare to its ſide.

It remains, therefore, now to be proved, that preciſely half of the mechanic power is *loſt* in the colliſion of non-elaſtic ſoft bodies; for which purpoſe my mind ſuggeſted the following reflections. In the colliſion of elaſtic bodies the effect ſeemingly inſtantaneous, is yet performed in *time;* during which time the natural ſprings reſiding in elaſtic bodies, and which conſtitute them ſuch, are bent or forced, till the motion of the ſtriking body is divided between itſelf and the body at reſt; and in this ſtate the two bodies would then proceed together, as in the caſe of non-elaſtic ſoft bodies; but as the ſprings will immediately reſtore themſelves in an equal time, and with the ſame degree of *impulsive force*, wherewith they were bent in this re-action, the motion that remained in the ſtriking body will be totally deſtroyed, and the total exertion of the two ſprings, communicated to the original reſting body, will cauſe it to fly off with the ſame velocity wherewith it was ſtruck.

Upon this idea, if we could conſtruct a couple of bodies in ſuch a way that they ſhould either act as bodies perfectly elaſtic; or that their ſprings ſhould at pleaſure be hooked up, retained, or prevented from reſtoring themſelves, when at their extreme degree of bending; and if the bodies under theſe circumſtances obſerved the laws of colliſion of non-elaſtic ſoft bodies, then it would be proved, that one half of the mechanical power, reſiding in the ſtriking body, would be loſt in the action of colliſion; becauſe the impulſive force or power of the ſpring in its reſtitution being cut off, or ſuſpended from acting, which is equal to the impulſive force or power to bend it (and which alone has been employed to communicate motion from one body to the other), it would make it evident, that one half of the impulſive force is loſt in the action, as the other half remains *locked up* in the ſprings, it alſo follows, as a *collateral circumstance*, that be the impulſive power of the ſprings what it may from firſt to laſt, yet as one half of the *time* of the action is by this means cut off, in this ſenſe alſo it will follow, that one half of the mechanic power is deſtroyed; or rather, in this caſe, remains locked up in the ſprings, capable of being *re-exerted* whenever they are ſet at liberty, and of producing a freſh mechanical effect, equivalent to the motion or mechanical power of the two non-elaſtic ſoft bodies after their colliſion.

Hence we muſt infer, that the quantity of mechanical power expended in diſplacing the parts of non-elaſtic ſoft bodies in colliſion, is exactly the ſame as that expended in

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in bending the ſprings of perfectly elaſtic bodies; but the difference in the ultimate effect is, that in the non-elaſtic ſoft bodies, the power taken to diſplace the parts will be totally loſt and deſtroyed, as it would require an equal mechanic power to be raiſed a-freſh, and exerted in a contrary direction to reſtore the parts back again to their former places; whereas, in the caſe of the elaſtic bodies, the operation of half the mechanic power is, as obſerved already, only locked up and ſuſpended, and capable of being re-exerted without a further original acceſſion.

Theſe ideas aroſe from the reſult of the experiments tried upon the machine deſcribed in my ſaid tract upon mechanic power, and were alſo communicated to my very worthy and ingenious friend WILLIAM RUSSEL, Eſq. F. R. S. at the ſame time that I ſhewed him thoſe experiments in 1759; but the mode of putting this matter to a full and fair mechanical trial has ſince occurred; and though ſome rough trials, ſufficient to ſhew the effect, were made thereon, prior to the offering the Paper on Mechanical Power to the Society in 1776, yet the machine itſelf I had not leiſure to complete to my ſatisfaction till lately; which I mention to apologize for the length of time that theſe ſpeculations have taken in bringing forward.

Deſcription of the Machine for Collision.

PLATE VIII. Fig. 1. ſhews the front of the machine as it appears at reſt when fitted for uſe.

A is the pedeſtal, and A B the pillar, which ſupports the whole, C D are two compound bodies of about a pound weight each, but as nearly equal in weight as may be. Theſe bodies are alike in conſtruction, which will be more particularly explained by fig. 2. Theſe bodies are ſuſpended by two white fir rods of about half an inch diameter, *e f* and *g h* being about four feet long from the point of ſuſpenſion to the centre of the bodies; and their ſuſpenſion is upon the croſs piece I I, which is mortiſed through, to let the rods paſs with perfect freedom; and they hang upon two ſmall plates filed to an edge on the under ſide, and paſs through the upper part of the rods. Their centres are at *k* and *l*, and the edges being let into a little notch, on each ſide the mortiſe, the rods are at liberty to vibrate freely upon their reſpective points (or rather edges) of ſuſpenſion, and are determined to one plane of vibration. M N is a flat arch of white wood, which may be covered with paper, that the marks thereupon may be more conſpicuous.

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The croſs piece I I is made to project ſo far before the pillar, that the bodies in their vibrations may paſs clear of it, without danger of ſtriking it; and alſo the arch M N is brought ſo far forward as to leave no more than a clearance, ſufficient for the rods to vibrate freely without touching it.

Fig. 2, ſhews one of the compound bodies, drawn of its full ſize. A B is a block of wood, and about as much in breadth as it is repreſented in height, through a hole in which the wood rod C C paſſes, and is fixed therein.

D B repreſents a plate of lead about three-eighths of an inch thick, one on each ſide, ſcrewed on by way of giving it a competent weight, *d* B *e f g* repreſents the edge of a ſpringing plate of braſs, rendered elaſtic by hard hammering; it is about five-eighths of an inch in breadth, and about one-twentieth of an inch thick. It is fixed down upon the wooden block at its end *d* B by means of a bridge plate, whoſe end is ſhewn *h i*, and is ſcrewed down on each ſide of the ſpring plate by ſcrews, which being relaxed the ſpring can be taken out at pleaſure, and adjuſted to its proper ſituation. *k l* is a light thin ſlip of a plate, whoſe under edge is cut into teeth like a fine ſaw or ratchet, and is attached to the ſpring by a pin at *k*, which paſſes through it, and alſo through a ſmall ſtud rivetted into the back part of the ſpring, and upon which pin, as a centre, it is freely moveable. *m n* ſhews a ſmall plate or ſtud ſeen edgeways raiſed upon the bridge plate, through an hole in which ſtud the ratchet paſſes; and the lower part of the hole is cut to a tooth ſhaped properly to catch the teeth of the ratchet, and retain it together with the ſpring at any degree to which it may be ſuddenly bent; and for this intent it is kept bearing gently downward, by means of a wire ſpring *o p q*, which is in reality double, the bearing part at *o* being ſemi-circular; from which branching off on each the rod C C, paſſes to *p*, and fixes at each end into the wood at *q*. However, to clear the ratchet, which is neceſſarily in the middle as well as the rod, the latter is perforated; and alſo the block is cut away, ſo far as to ſet the mainſpring at *e* free of all obſtacles that would prevent its play from the point B. The part *f g* is ſhewn thicker than the reſt, by being covered with thin kid leather tight ſewed on, to prevent a certain jarring that otherwiſe takes place on the meeting of the ſprings in colliſion.

Let us now return to fig. 1, the marks upon the arch M N are put on as follows: *o p* is an arch of a circle from the centre *l*, and *q r* an arch of a circle from the centre *k*, interſecting each other at S. Now the middle line of the marks *t*, *v*, are at the ſame diſtance from the middle line at S that the centres *k l* are; ſo that when each

each body hangs in its own free poſition, without bearing againſt the other, the rod *e f* will cover the mark at *t*, and the rod *g h* will cover the mark at *v*. From the point S upon the arches S *p* and S *q* reſpectively, ſet off points at an equal and competent diſtance from S each way, which will give the middle of the mark *w* and *x*: and upon the arch S *p* find the middle point between the mark *v* and *w*, which let be *y*; and on the other ſide, in like manner, upon the arch S *q* find a middle point for the mark *x*; then ſet off the diſtance S *v* or S *t* from *y* each way, and from *z* each way; and from theſe points, drawing lines to the reſpective centres *l* and *k*, they will give the place and poſition of the marks *a*, *b*, and *c*, *d*; and thus is the machine prepared for uſe.

For Trials on elastic Bodies:

For this uſe take out the pins and ratchets from each reſpectively, and the ſprings being then at liberty, with a ſhort bit of ſtick (ſuppoſe the ſame ſize as the rods) turn aſide the rod *g h* with the right hand, carrying the body D upwards till the ſtick is upon the mark *w*, as ſuppoſe at ⊙; there hold it, and with the left ſet the body C perfectly at reſt; in which caſe the rod *e f* will be over the mark *t*; then ſuddenly withdraw the ſtick, in the direction that the rod *g h* is to follow it, and the ſpring of the body D, impigning upon that of the body C, they will be both bent, and alſo reſtored; and the body C will fly off, and mount till its rod *e f* covers the mark *x*; the rod of the ſtriking body D remaining at reſt upon its proper mark of reſt *v*, till the body C returns, when the body D will fly off in the ſame manner; the two bodies thus rebounding a number of times, loſing a part of their vibration each time; but ſo nearly is the theory of elaſtic bodies fulfilled hereby, that the ſingle advantage of originally puſhing the rod *g h* beyond the mark *w*, by the thickneſs of the ſtick, or its own thickneſs, is ſufficient to carry the rod of the quieſcent body C completely to its mark *x*.

There are ſeveral other experiments which may be made with this apparatus, in confirmation of the doctrine of the colliſion of elaſtic bodies; which being univerſally agreed upon, and well known, it is needleſs further to dwell upon here; but reſpecting the application to non-elaſtic ſoft bodies, it is far more difficult to come at a fitneſs of materials for this kind of experiment, than it is for thoſe ſuppoſed perfectly elaſticity. The concluſions, however, may be attained with equal certainty.

For

For Trials on Non-elastic soft Bodies:

For this purpose the ratchets must be applied and put in order as before described, and the springs being both put to their point of rest, let the body D be put to its mark *w* in the same manner as before described, and the body C to rest. The body D being let go, and striking the body C at rest, in consequence of the stroke, the springs being hooked up by the ratchets, they both move from their resting marks *t*, *v*, respectively towards M: Now if they both moved together, and the rod *e f* covered the mark *c*, and the rod *g h* covered the mark *d* at their utmost limit, then they would truly obey the laws of non-elastic soft bodies; because their medium ascent would be to the mark *z*, which is just half the angle of ascent to the mark *x*; but as in this piece of machinery, though the main or principal springs are hooked up, yet every part of them, and all the materials of which they are composed, and to which they are attached, have a degree, or more properly speaking, a certain compass of *elasticity*, which, as such, is perfect, and no motion is lost thereby.

We must not, therefore, expect the two compound bodies after the stroke to stick together without separating, as would be the case with bodies truly non-elastic and soft; but that from the elasticity they are possessed of, they will by rebounding be separated; but that elasticity being perfect, can occasion no loss of motion to the sum of the two bodies; so that if the body C ascends as much above its mark *c* as the body D falls short of its mark *d*, then it will follow, that their medium ascent will still be to the mark *z*, as it ought to have been, had they been truly non-elastic soft bodies; and this, in reality, is truly the case in the experiment, as nearly as it can be discerned.

After a few vibrations, by the rubbing of the springs against one another, they are soon brought to rest; and here they would *always rest* had they been truly and properly perfect non-elastic soft bodies; but here, as in the case of these bodies, by a change of the figure and situation of the component parts, there is expended one half of the mechanical power of the first mover, yet in this case the other half is not *lost*, but *suspended* ready to be re-exerted whenever it is set at liberty; and that it is really and *bonâ fide one half*, and neither more nor less appears from this uncontroverted simple principle, that the power of restitution of a perfect spring is exactly equal to the power that bends it. And this may, in a certain degree, be shewn to be fact by experiment, if there were any need of such a proof; for if, when the bodies are at rest after the last experiment, the two rods are lashed together at the bottom with a bit of thread, and then

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the ratchets unpinned and removed; on cutting the thread with a pair of ſciſſars they will each of them rebound, C towards M and D towards N, and if they rebounded reſpectively to x and y, the mechanical power exerted would be the ſame as it was after the ſtroke, when the mean of their two aſcents was up to the mark Z; but here it is not to be expected, becauſe not only the motion loſt by the friction of the ratchets is to be deducted, becauſe it had the effect of real non-elaſticity; but alſo the elaſticity that ſeparated them in the ſtroke, which was loſt in the vibrations that ſucceeded; neither of which hindered the mean aſcent to be to z; but yet, under all theſe diſadvantages in the machine (if not unreaſonably ill made) the rod $e f$ will aſcend to d, and $g h$ to a: and hence I infer, as a poſitive truth, that in the colliſion of non-elaſtic ſoft bodies, *one half of the mechanic power residing in the striking body is lost in the stroke.*

Reſpecting bodies unelaſtic and perfectly hard, we muſt infer, that ſince we are unavoidably led to a concluſion concerning them, which contradicts what is eſteemed a truth capable of the ſtricteſt demonſtration; viz. that the velocity of the centre of gravity of no ſyſtem of bodies can be changed by any colliſion betwixt one another, ſomething muſt be aſſumed that involves a contradiction. This perfectly holds, according to all the eſtabliſhed rules, both of perfectly elaſtic and perfectly non-elaſtic *ſoft* bodies; rules which muſt fail in the perfectly non-elaſtic *hard* bodies, if their velocity after the ſtroke is to the velocity of the ſtriking body as one is to the ſquare root of 2; for then the centre of gravity of the two bodies will by the ſtroke acquire a velocity greater than the centre of gravity the two bodies had before the ſtroke in that proportion, which is proved thus.

At the outſet of the ſtriking body, the centre of gravity of the two bodies in our caſe will be exactly in the middle between the two; and when they meet it will have moved from their half diſtance to their point of contact, ſo the velocity of the centre of gravity before the bodies meet will be exactly one half of the velocity of the ſtriking body; and, therefore, if the velocity of the ſtriking body is 2, the velocity of the centre of gravity of both will be one. After the ſtroke, as both bodies are ſuppoſed to move in contact, the velocity of the centre of gravity will be the ſame as that of the bodies; and as their velocity is proved to be the ſquare root of 2, the velocity of their centre of gravity will be increaſed from 1. to the ſquare root of 2; that is, from 1. to 1.414, &c.

The fair inference from theſe contradictory concluſions therefore is, that an unelaſtic hard body (perfectly ſo) is a repugnant idea, and contains in itſelf a contradiction; for,

to make it agree with the fair concluſions that may be drawn on each ſide, from clear premiſes, we ſhall be obliged to define its properties thus: that in the ſtroke of unelaſtic hard bodies they cannot *possibly lose any* mechanic power in the ſtroke; becauſe no other impreſſion is made than the communication of motion; and yet they *must lose* a *quantity* of mechanic power in the ſtroke; becauſe, if they do not, their common centre of gravity, as above ſhewn, will acquire an *increase* of velocity by their ſtroke upon each other.

In a like manner the idea of a *perpetual motion* perhaps, at firſt ſight, may not appear to involve a contradiction in terms; but we ſhall be obliged to confeſs that it does, when, on examining its requiſites for execution, we find we ſhall want bodies having the following properties; that when they are made to *aſcend* againſt gravitation their abſolute weight ſhall be *less;* and when they *deſcend* by gravitation (through an equal ſpace) their abſolute weight ſhall be greater; which, according to all we know of nature, is a *repugnant* or *contradictory idea.*

A DESCRIPTION

A DESCRIPTION of a new Tackle or Combination of Pullies, by Mr. JOHN SMEATON.

Philosophical Transactions, VOL. XLVII.

Read June 11, 1752. THE axis *in peritrochio*, and the tackle of pullies, are the only mechanic powers, which can, with convenience, be applied to the moving of large weights, when the height, to which they are intended to be raiſed, is conſiderable. The excellence of the former is, its working with little friction; that of the latter, its being eaſily moved from place to place, and applied *ex tempore*, as occaſion requires; but when the weights are very large they are uſed in conjunction.

The preſent methods of arranging pullies in their blocks may be reduced to two *. The firſt conſiſts in placing them one by the ſide of another, upon the ſame pin; the other in placing them directly under one another, upon ſeparate pins. But in each of theſe methods an inconvenience ariſes, if above three pullies are framed in one block. For, according to the firſt method, if above three pullies are placed by the ſide of one another, as the laſt part of the rope by which the draught is made (or, as it is commonly called, the fall of the tackle) muſt neceſſarily be upon the outſide pulley or ſhieve; the difference of the friction of the pulley will produce ſo great a tendency to pull the block awry, that as much will be loſt by the rubbing of the ſhieves againſt the block, on account of its obliquity, as will be got by increaſing the number of pullies.

The ſecond method is free from this objection; but, as the length of the two blocks, taken together, muſt be equal to the ſum of the diameters of the pullies, beſides the ſpaces between for the ropes, and the neceſſary appendages of the framing; if there are more than three pullies in each block, they will run out into ſuch an inconvenient length, as to deduct very conſiderably from the height, to which the weight might otherwiſe have been raiſed: ſo that, upon thoſe accounts, no very great purchaſe can be made by the common ſimple tackles of pullies alone.

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* This is to be underſtood of ſimple tackles or purchaſes by pullies, where one rope line or fall is reeved round and common to all.

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In order, therefore, to increaſe their power on board a ſhip, a purchaſe is commonly made uſe of for loading and unloading of goods, called a *runner and tackle*; by this method the power of any ſimple tackle is doubled by the runner, and is very commodious there, as the maſts afford a ſufficient height; but as the runner block, upon which the whole weight depends, muſt be fixed at more than double the height to which the body is intended to be raiſed it is ſeldom uſed aſhore.

Sometimes a ſecond tackle is fixed upon the fall of the firſt; but here it is obvious, that whatever be the power of the ſecond tackle, the height to which the weight might otherwiſe have been raiſed by the firſt, will be leſs in the ſame proportion as the purchaſe is increaſed by the ſecond; unleſs it is ſhifted, which loſes time.

Again, very frequently the fall of the firſt tackle is applied to a capſtan or windlaſs, which increaſes the purchaſe very commodiouſly without the inconveniencies laſt-mentioned; but then the machine is rendered more cumberſome, and, conſequently, leſs fit for a moveable apparatus.

Thoſe impediments I have avoided, by combining the two methods of managing pullies firſt deſcribed. The pullies in this new combination are placed in each block in two tiers; ſeveral being upon the ſame pin, as in the firſt method, and every one having another under it, as in the ſecond; as alſo that, when the tackle is in uſe, the two tiers, that are the remoteſt from one another, are ſo much larger in diameter than thoſe that are neareſt, as to allow the lines of the former to go over the lines of the latter without rubbing.

From this conſtruction ariſes a new method of reeving the line: for here, let the number of ſhieves be what it will, the fall of the tackle will always be upon the middle ſhieve, or on that next the middle, according as the number of pullies on each pin is odd or even.

To do this, the line is fixed to ſome convenient part of the upper block, and brought round the middle ſhieve of the larger tier of the under block, or the ſhieve neareſt to the middle if the number is even, from thence round one of the ſame ſort, *next* to the centre one, of the upper block; and ſo on till the line comes to the outſide ſhieve, where the laſt line of the larger tier falls upon the firſt ſhieve of the ſmaller, and being reeved round thoſe, till it comes to the oppoſite ſide; the line from the laſt ſhieve of the ſmaller tier, again riſes to the firſt of the larger, whence it

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is conducted round the remainder of the larger tiers till it ends on the middle ſhieve of the upper block; on the larger tier, or the ſhieve neareſt to the middle, if the number is even, as will appear more plain, by inſpection of figure 6, PLATE I.

In this method all the lines or parts are clear of one another, and the friction being more equally divided, the blocks are kept parallel.

The model in braſs which I have the honor to ſhew the Society, and from which I made the draught, conſiſts of 20 ſhieves, five on each pin. With this model, which may eaſily be carried in the pocket, I have raiſed ſix hundred weight. But with a tackle of this ſort, properly executed in large, one man will eaſily raiſe a ton, and a greater number of men a greater weight, in proportion *.

I have tried ſeveral numbers of ſhieves as far as 36; but 20 ſeems to be the largeſt number that will anſwer well in practice.

By this tackle all moderate weights (that is, from ſix hundred weight to two or three tons) can be raiſed to any moderate height, without being compounded with any other purchaſe, and without any loſs of height more than the length of the two blocks, which is common to all purchaſes with pullies whatever; and without requiring more room than the ſpace neceſſary for the aſcent of the body: this tackle is eaſily moved from place to place, and where neceſſary, can be applied with equal advantage in compound purchaſes, in the ſame manner as any other ſimple tackle.

Large weights can alſo be raiſed by bringing the fall of the tackle to a capſtan or windlaſs, and weights three times heavier than can be raiſed in the common way, may be raiſed by this method, becauſe the ſize of the largeſt rope being given, that can be uſed as a tackle fall, 20 of thoſe ropes will bear more than three times the weight that ſix will bear; and laſtly, the weight being given, the tackle fall may be leſs in the proportion of 20 to 6, which will render the whole more flexible, and as the block pins can be conſiderably leſs, the friction will be leſs alſo, and the tackle more eaſily over hauled.

* A large tackle of 20 was tried on board one of his majeſty's ſhips; and by the help thereof, though it was with a new rope, one man raiſed one of the ſhip-guns and carriage, that together weighed 27 hundred weight; there being a perſon, as uſual, to *hold on*, or prevent the rope from ſlipping back.

A very

A very commodious tackle of 12 may be executed in wood, in the ſame manner that common blocks are made *.

I ſhould not have troubled the Royal Society with an account of this contrivance, did it not ſeem promiſing of ſome utility, in a variety of purpoſes; particularly for merchants, ſeamen, builders, engineers, &c. I therefore entirely ſubmit it to the cenſure of that honorable body.

J. SMEATON.

P. S. In conſtructing in wood a tackle of 20 for 3 tons, the larger tier of ſhieves ſhould not be leſs than 10 inches; the running line needs not be more than 2 inches in circumference, and the pins, if iron, not exceeding five-eighths, if ebony, three-fourths of an inch in thickneſs.

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* All the principal purchases used in building *Edystone Light-house*, with *stone*, are of this kind, and which are subject to no material inconvenience, but, on the contrary, for wet and dry, and rough usage, wooden pins, and holes both turned true and smooth, seem preferable to Mr. HALL, the same hand as also executed a large tackle of 20 in wood with success.

A DISCOURSE

A DISCOURSE concerning the Menstrual Parallax, arising from the mutual Gravitation of the Earth and Moon, its Influence on the Observation of the Sun and Planets, with a Method of observing it, by J. SMEATON, F. R. S.

Read May 12, 1768.

IT is demonstrated by Sir ISAAC NEWTON, in the *Principia*, that it is not the earth's centre, but the common centre of gravity of the earth and moon, that describes the ecliptic; and that the earth and moon revolve in similar ellipses, about their common centre of gravity.

The same great author has also investigated, from the different rise of the tides, when the moon is in conjunction or opposite to the sun, to those which happen when the moon is in her quadratures; that the quantity of the matter in the earth is to that in the moon, as 39, 78 to 1; from whence, and the known distance of the earth and moon, it would follow, that the common centre of gravity of the two bodies falls without the surface of the earth, by one half of its semi-diameter, that is, that the centre of the earth describes an epicycle round the common centre of gravity once a month, whose diameter is three semi-diameters of the earth.

Dr. GREGORY, in his astronomy, has laid hold of this circumstance, in order to prove the relative gravity of the earth and moon, by observation; which is the subject of his sixtieth proposition of the fourth book; in which he has demonstrated, that if an observer on the earth makes a correct observation on the sun's place, when the moon is in one quadrature, it will differ from a like observation, taken in the opposite quadrature (according to a mean elliptic motion) by an angle which the diameter of this epicycle will subtend at the sun. The same learned author has also shewn, in the scholium to the same proposition, that this quantity, or parallax, will be twice greater to Mars in opposition, and three times greater to Venus, in her inferior conjunction with the sun.

The difference thus produced in the apparent place of the sun, and of all the primary planets, being governed by the moon, and having its period the same, may perhaps not be unaptly called *menstrual parallax*.

Now, if, with Sir ISAAC NEWTON, the relative gravities of the earth and moon are taken between the proportion of 39 and 40 to one, the menstrual parallax of the sun will

will come out 13″ upon the radius of the earth's epicycle, and will affect the ſolar obſervations at the oppoſite quadratures, by double that quantity, viz. 26″ in like manner, the mean diſtance from the earth of Mars in oppoſition, being to the ſun's mean diſtance as 1 to 2.1; and the leaſt diſtance of Mars from the earth to the ſun's mean diſtance, as 1 to $2\frac{3}{4}$, the menſtrual parallax of Mars will affect the obſervation upon him in that ſituation, by 56″ and $73''\frac{1}{2}$ reſpectively.

The mean diſtance of Venus from the earth, in her inferior conjunction, being to that of the ſun as $3\frac{1}{3}$ to 1 nearly, and not very variable, on account of the orbit of Venus being almoſt circular; the menſtrual parallax would affect the place of Venus in that ſituation, by a quantity not leſs than 92″; and in all other ſituations in proportion to her diſtance; which alſo holds with reſpect to all the reſt of the planets.

Theſe diſturbing quantities are by no means to be diſpenſed with in the nice and critical ſtate that aſtronomical obſervations and calculations have arrived at, in conſequence of the diſcoveries of Dr. BRADLEY, who may be ſaid to have given a baſis to aſtronomy; however, could we rely upon the *data*, on which Sir ISAAC's inveſtigation of the relative gravity of the earth and moon is founded, we ſhould have nothing to do but to apply an equation to the particular caſes, according to the diameter of the epicycle, as deduced from the relative gravity; but whoever conſiders the great obſtructions that the water of the ſea meets with in its motion, to obey the influence of the moon; the great difficulty in aſcertaining the true height of the tides, from the many diſturbing cauſes intervening; and the many uncertainties, and the want of coincidence that have attended, and muſt attend ſuch obſervations; muſt confeſs that this matter does not ſeem capable of ſuch a determination from that quarter as the preſent ſtate of aſtronomy requires.

Accordingly, ſince the time of Dr. GREGORY, thoſe great aſtronomers Dr. BRADLEY, DE LA CAILLE, and others, have applied themſelves to determine the quantity of the menſtrual parallax, from ſolar obſervations; but though theſe have given cauſe to ſuppoſe that the relative gravity of the earth and moon are not above $\frac{2}{3}$ of the quantity deduced from the tides; yet, as the obſervation of theſe ſmall angles principally depends upon the obſervation of the ſun's right aſcenſion (which, depending on the meaſure of time, is leſs capable of exact obſervation, than if depending on divided inſtruments); the deductions thence drawn ſeem ſtill wanting of that certainty which the ſubject demands; and if to this we add, from a deduction of Mr. MASKELYNE, that the relative gravity of the earth and moon is as 76 to 1, derived from the effect that the moon produces

produces in the nutation of the earth's axis; the relative gravity, and, conſequently, the parallaxes thereon depending, will be reduced to one half of thoſe reſulting from Sir ISAAC's determination.

It is true, that the quantity of effect of the menſtrual parallaxes will not be great, if computed upon Mr. MASKELYNE's induction, for as much as that the common centre of gravity will be conſiderably within the earth's ſurface; yet, even in that caſe, the ſun's tranſit over the meridian, when the moon is in one quadrature, will differ nearly one ſecond of time from that obſerved from the oppoſite quadrature; and though DE LA CAILLE and MAYER have formed equations depending on the moon, to be applied to the equation of time; yet, if we are at an uncertainty, whether the *maximum* is one ſecond, two thirds of a ſecond, or half a ſecond of time, each way, we are ſtill under a material difficulty; for though theſe differences are ſo ſmall, that it is not eaſy to determine them exactly from ſolar obſervations; yet, as they are capable of creating a ſenſible difference in theſe obſervations, they will, ſo long as they remain undetermined, prevent that ſolidity and firmneſs to the ſolar obſervations, which is the more neceſſary as they are the foundation of all the reſt; but with reſpect to thoſe planets, that in their periods come nearer to us than we to the ſun, the obſervations upon them will be effected by a greater uncertainty.

The determination of the menſtrual parallax is of ſtill more importance, as it is a neceſſary conſideration in the determination of the ſun's parallax; and this, whether deduced from Mars or Venus, as I ſhall preſently ſhew more particularly; but at firſt I muſt ſtate the quantity of the menſtrual parallax, according to the beſt *data* yet known, by a contrary proceſs; and, taking the mean quantity of the ſun's parallax according to the determination of Mr. SHORT, at 8″ 8, and the relative gravities of the earth and moon, according to Mr. MASKELYNE, as 76 to 1, and the mean diſtance of their centres equal to 60½ ſemi-diameters, we ſhall then have the diſtance of the earth's centre from the centre of gravity, at $\frac{8}{176}$ of the earth's ſemi-diameter, (that is, $\frac{1}{22}$ of that ſemi-diameter within the earth's ſurface), and the menſtrual parallax equal to $\frac{8}{176}$ of the ſun's parallax; conſequently about 7″; and the double menſtrual parallax, or vacillation, ariſing from the whole diameter of the epicycle 14″; the mean menſtrual parallax of Mars in oppoſition 29″½; the greateſt 38″½; and that of Venus 49″; from hence it follows, that, were a perſon to attempt the ſun's parallax, by the diurnal motion of the earth, applied as a baſis to Mars in oppoſition, as has been formerly tried; and ſhould the moon be at new or full at the ſame time, the change of place of the earth's centre, in its own epicycle, would amount to an angle ſeen from Mars of 1″ 3 nearly; that is,

in caſe the interval between the obſervations were eight hours, and Mars at his mean diſtance, this change would in the ſame time amount to $1''.7$ nearly. In like manner, if a tranſit of Venus happen near the new or full moon (as will be the caſe next year), the time of the tranſit will be effected by a change of place, ſuch as the earth's centre will deſcribe in its epicycle during the time of the whole tranſit, if the beginning and end are obſerved in the ſame place, or during the difference of abſolute time, at which the tranſit appears to begin or end to different obſervers in diſtant meridians. Thus, when the ſame obſerver ſees the beginning and end in the ſame place, the caſe deſcribed by that obſerver, from the earth's diurnal motion, muſt be corrected by the ſpace deſcribed by the earth's centre, in the circumference of its epicycle, during that time; which, if it be ſuppoſed of ſeven hours, will amount to an angle $1''.9$, ſeen from Venus: but, where the beginning or end is ſeen by different obſervers in diſtant meridians, as the difference of abſolute time can hardly amount to above fifteen minutes, the change of place of the earth's centre will for that time be but ſmall; however, at the rate before mentioned, it will for fifteen minutes affect the parallactic angle ſeen from Venus by about $\frac{7}{100}$ of a ſecond; and the parallax of the ſun, by about $\frac{1}{100}$ part of the whole: but this proportional part will remain the ſame, whether the diſtance of meridians be ſuch as produce a greater or leſs difference of abſolute time than fifteen minutes *.

From what has been ſaid, I ſuppoſe it will appear, that the effects of the menſtrual parallax are worthy of conſideration, and that nothing has been yet executed, whereby it has received a determination ſufficiently accurate; for, in regard to obſervations upon the ſun, the whole quantity is too ſmall to be minutely obſerved in right aſcenſion; and with reſpect to the application to Mars and Venus, has not been as yet ſo critically reduced to computation, as to render their parallaxes (though in themſelves much greater) deducible with equal certainty as that of the ſun.

What I have now to propoſe, is a method of obſerving the menſtrual parallaxes of Mars and Venus, without laying an undue ſtreſs upon the theory of their motions.

The firſt opportunity of making an obſervation for this purpoſe, will be at the next oppoſition of Mars, which, according to the nautical almanack, will happen the 26th of

* If an error of $\frac{1}{200}$ part of the whole may be ſuppoſed in the obſervation for determining the ſun's parallax by the tranſit of Venus, a neglect of the menſtrual parallax may make it $\frac{1}{200}$ part of the whole.

October

October next, in the morning; I will therefore endeavour to illuſtrate this matter by taking that as an example.

The diſtance of Mars from the earth will then be ſomewhat leſs than the mean diſtance, that is, as 1 to 2.2; and, conſequently, his double menſtrual parallax, according to Mr. MASKELYNE, will be nearer $31''$ in the point of oppoſition. Now, as the moon will be at full not above twelve hours preceding that oppoſition, the moon will be nearly in the moſt favourable ſituation for the purpoſe.

For this end, let an accurate obſervation be made upon the place of Mars at the following times, viz. firſt, near the time of the new moon, preceding Mars's oppoſition, or more properly at the neareſt opportunity, to the time of the moon's oppoſition to Mars, which will happen in the night, between the 12th and 13th of October; ſecondly, let the place of Mars be obſerved when the moon is neareſt her quartile with Mars; that is, between the 19th and 20th of the ſame month; thirdly, let an obſervation be made on Mars when the moon is in conjunction with Mars, the neareſt to his oppoſition with the ſun, that is, between the 25th and 26th of October; fourthly, let Mars be again obſerved when the moon has moved on to her quartile with Mars, viz. between the 31ſt of October and 1ſt of November; and fifthly and laſtly, let the place of Mars be obſerved when the moon has again got to her oppoſition with Mars, which happens between the 7th and 8th of November.

Now it is manifeſt, that when the moon is in conjunction or oppoſition to Mars, the centre of the earth, the centre of Mars, and the common centre of gravity of the earth and Mars, will be nearly in a right line, and, conſequently, that an obſerver will then ſee Mars, in the ſame place in the heavens, as if the common centre of gravity were the ſame as the centre of the earth; therefore, the place of Mars will then be unaffected by a menſtrual parallax; and ſuch will be the firſt, third, and fifth, of the obſervations above propounded.

It is equally evident, that when the moon is in quartile with Mars, and moving towards a conjunction, an obſerver, at the earth's centre, will ſee Mars more backward in the ecliptic, than if ſeen from the common centre of gravity, by $15''\frac{1}{2}$; and that, when the moon is in her oppoſite quartile with Mars, and moving from her conjunction, an obſerver at the earth's centre will ſee Mars advanced in his orbit more forward by $15''\frac{1}{2}$, than if ſeen from the common centre of gravity; and the one obſervation

tion checked with the other, will, according to a mean elliptic motion, differ by the quantity of 31″; and ſuch will be the ſecond and fourth obſervations above propounded.

Now, from the firſt, third, and fifth obſervations, three points of Mars' orbit will be given; which, by the help of the theory of Mars's motion in an elliptic orbit, whoſe aphelion, eccentricity, and nodes, are known ſufficiently near for this purpoſe, the intermediate places of Mars may be inferred with the requiſite degree of accuracy; and particularly, as the two intermediate obſervations, viz. the ſecond and fourth, will be nearly at equal intervals of time between the three others: from hence it follows that the difference between the inferred or computed places, at the quartiles, and the obſerved places at thoſe times will be the menſtrual parallax required.

It is to be noted, that the times above ſpecified are the moſt favourable for the obſervation; and could thoſe be made uninterruptedly from weather, there would be the leſs occaſion for any other; but as much as poſſible to prevent diſappointment of this kind, it will be right to begin the obſervations a month preceding, making the proper obſervations, at the conjunctions, quartiles, and oppoſitions of the moon with Mars, which will be the means of ſupplying ſuch obſervations as may happen to prove abortive before the oppoſition of Mars, and alſo, in caſe any of the obſervations to be made after that oppoſition ſhall prove deficient, the obſervations may be carried on a month or competent time afterwards.

As a further ſecurity againſt diſappointments, as well as check, it will alſo be adviſable to make proper obſervations the night preceding, and ſubſequent to thoſe in which the quartiles, conjunction, &c. happen; for, as the quantities will not differ conſiderably from thoſe obtained on the days ſpecified, with proper allowances they may be brought in ſupport and confirmation of the former.

In like manner, when Venus is moving towards her inferior conjunction with the ſun, as will happen next year, the ſame obſervations may be made with reſpect to her, and continued for a neceſſary time to get obſervations of the place of Venus; viz. the firſt, when the moon is in conjunction or oppoſition with Venus: a ſecond, when the moon is in her quartile with Venus; a third, in conjunction or oppoſition; a fourth, when the moon is in her oppoſite quartile to the former; and a fifth, again in conjunction or oppoſition:

poſition: the ſame opportunity will alſo offer when Venus is moving from her inferior conjunction with the ſun, and becomes a morning ſtar.

In regard to the obſervation of Venus, it is remarked by aſtronomers, that ſhe is to be ſeen with a good tranſit teleſcope, when ſhe is within a few degrees of the ſun; but as ſhe is three times nearer the earth, than the ſun's mean diſtance, when her elongation is 25° in the inferior part of her orbit, it is plain, that the neceſſary obſervations may be eaſily made when her menſtrual parallax will be at a medium, three times greater than the ſun's; and, conſequently, amounting for the whole difference 42″.

To avoid embarraſſment in deſcription, I have hitherto ſuppoſed that all the obſervations are made in the meridian; in which caſe the right aſcenſions will be the ſame as they would appear from the centre of the earth, and, conſequently, the planet's longitudes thence deduced nearly the ſame; but 'tis eaſy to ſee, that if the quartile obſervations are made when the planets are conſiderably to the eaſt or weſt of the meridian, and ſo choſen, that the place of the obſerver be further diſtant from the common centre of gravity, than the centre of the earth is from that centre, that the caſe of the obſervations will be conſiderably enlarged. Thus, in our latitude, ſuppoſing that the quartile obſervations are made four hours before and four hours after the planet paſſes the meridian, this will produce an enlargement of the baſis by one of the earth's ſemi-diameters; and as the whole baſe or diameter of the epicycle comes out, according to Mr. MASKELYNE, no more than 1. 6 of the earth's ſemi-diameters, the baſe will, according to this method, come out 2. 6; and, conſequently, at the next oppoſition, the menſtrual parallax of Mars will be thereby enlarged to 50″, the greateſt to 62″½, and that of Venus at a mean to 74″½.

It muſt however be acknowledged, that no kind of obſervations of the places of the planets are of equal validity with thoſe taken with the beſt inſtruments in the meridian; thoſe taken with micrometers perhaps not excepted; for, however accurately ſmall diſtances can be meaſured with the micrometer of Mr. Dolland, yet, as theſe meaſures can hardly be reduced to the ecliptic, without having the difference of declination or right aſcenſion from other means, (except two ſtars making ſomewhat near a right angle with the planet, ſhould appear within the field of view at once); and as in all theſe caſes the rectification of the places of the ſtars themſelves ultimately depends on meridian obſervations, we may perhaps be allowed to ſay, that in the moſt favorable caſes of the micrometer, the determinations thence to be drawn are not

not ſuperior to meridian obſervations, and in leſs favorable caſes muſt be inferior; however, as the micrometer obſervations out of the meridian give an opportunity of repetition as often as we pleaſe, and the obſervations for the rectification of the ſtars concerned, can be repeated in the meridian as often as we pleaſe alſo, it muſt be equally allowed, that when theſe kind of obſervations are taken, not too near the horizon, when proper ſtars offer for this purpoſe, and the whole ſkilfully managed, theſe kind of obſervations fall but little ſhort of thoſe taken immediately in the meridian. I cannot therefore heſitate to recommend, that the quartile obſervations be taken out of the meridian, as well as in it in the firſt place, by Dolland's micrometer, if ſtars offer in proper poſitions, and if not, ſecondly, by taking differences of right aſcenſion and declination between the planet and the ſtars, by the common micrometer, in caſe proper ſtars offer themſelves for this purpoſe; but as it frequently happens, that no proper ſtars offer themſelves to micrometers of either kind, and this is ſtill more likely to happen in the obſervations of Venus, which will be chiefly in the day light, I beg leave to offer (what to me is) a new method of obſervation out of the meridian; and which, though I eſteem it not equal to micrometer obſervations of either kind, I apprehend will fall ſo little ſhort thereof, and prove ſo much ſuperior to any other method now in practice in theſe caſes, that I hope I ſhall, on this occaſion, be excuſed in giving a particular deſcription thereof; but, as it is a general method of obſerving out of the meridian, I ſhall reſerve it by way of appendix.

In the next obſervation of Mars, it has been ſtated that, in the meridian obſervations alone, the menſtrual parallax, according to the ſmalleſt eſtimation, may be expected to amount to $31''$ in longitude, which, turned into right aſcenſion, will make about $2''$ of time: now, if it may be allowed, that a well practiſed obſerver can take the time of a tranſit to one quarter part of a ſecond, over a ſingle wire, if he has three wires, or more, as uſual, the mean of the three ſhould be within $\frac{1}{12}$ part of a ſecond; or within $\frac{1}{12}$ part of the whole quantity in queſtion; it is, however, a matter of chance, whether the mean of three may or may not be within one-third part of the whole; and, as equal errors may be committed in the obſervations of the tranſits of the ſtars, wherewith the right aſcenſions of the planets in queſtion are compared, which it is an equal chance, whether they tend to correct or increaſe the errors committed in the former, yet, if, as has already been propoſed, the obſervations are continued for two or three months, inſtead of one, and obſervations, taken the preceding day and ſubſequent to the days of conjunction, quartile, and oppoſition; and this, as well out of the meridian as in it, we can hardly doubt, but that, if the weather ſhould favor, ſo many checks would be formed, that, from the next oppoſition of Mars alone, the affair

affair may be brought within a twenty-fourth part of the whole; and, if to this be added the force of ſuch determinations, as may be drawn from obſervations on Venus, before and after her tranſit over the ſun next year, it can hardly be doubted, but that thoſe three will bring us within a ſingle ſecond of a degree, ſubtended from the neareſt planet; and theſe concluſions will be further ſtrengthened by future obſervations, as two years will ſcarcely paſs without affording one or more opportunities of this kind.

As I meant not to embarraſs myſelf with exact computations, I have conſtantly ſuppoſed the diſtance of the common centre of gravity from the centre of the earth, to be a fixed quantity, whereas it will vary in the ſame proportion as the moon's diſtance varies; but as this and many other minutiæ will properly enter the computation, when the obſervations are made, I muſt beg leave to refer them to the learned in this ſcience.

J. SMEATON.

Auſthorpe, April 17, 1768.

DESCRIPTION

DESCRIPTION of a new Method of observing the Heavenly Bodies, out of the Meridian, by J. SMEATON, F. R. S.

Read May 16, 1768.

THE inſtrument I propoſe for this purpoſe, is a tranſit teleſcope, mounted on a vertical axis; for example, ſuch a one as is deſcribed in the introduction to the Hiſtoire Celeſte, of Mr. Le Monnier, being one of the inſtruments made by Mr. Graham, for the academicians, who went to meaſure a degree at the polar circle; this, or any other inſtrument upon equivalent principles, will ſuffice, that is capable of ſuch adjuſtments as to be made correctly to deſcribe an almicanther and azimuth circle, and capable of being retained in any given poſition; the uſe will appear by the following example.

Make choice of any fixed ſtar, which, according to the diurnal motion, precedes the heavenly body to be obſerved by a few minutes, more or leſs, as it may happen; let the inſtrument be ſet to an azimuth, ſomewhat preceding the fixed ſtar; and carefully obſerve the time of the ſtar's tranſit croſs the vertical wire of the teleſcope; then wait till the heavenly body comes to the ſame azimuth; and, when arrived within the field of view, keep gently turning the ſcrew that alters the elevation of the teleſcope, ſo as to follow the heavenly body in altitude, keeping it interſected by the horizontal wire of the teleſcope, till the body paſſes the middle vertical wire, and carefully note the time of its paſſage; there leave the teleſcope fixed as to altitude, and releaſing the horizontal motion, turn it round on its vertical axis, till you meet with ſome ſtar, that in a little time after you will, by riſing or falling, come to the ſame almicanther; and, on its arrival, carefully note the time of its paſſage a croſs the horizontal hair of the teleſcope.

Now, from the right aſcenſions and declinations of the two ſtars being previouſly known, or afterwards determined from meridian obſervations, the azimuth of the firſt ſtar, and the altitude of the laſt, at the time of their reſpective paſſages, may be determined by computation, which will give the altitude and azimuth of the heavenly body, for the time of the middle obſervation, when it paſſed the interſection of the two wires.

The ſame end may alſo be obtained by taking the obſervations in an inverted order; that is, by chooſing a ſtar at ſuch an altitude, that the heavenly body ſhall, in a competent

petent time afterwards, arrive at the ſame altitude, &c. but, as in theſe latitudes the alteration of azimuth is, eſpecially in thoſe parts that are in the neighbourhood of the zodiack, quicker than that of the altitude, I apprehend it to be eaſier to follow the ſlower motion with the ſcrew, ſo as to preſerve the interſection, than the quicker, and therefore in general to be preferred; but where it happens otherwiſe, or the ſtars lie more conveniently, the inverſe method may be purſued.

It is true, that ſome degree of dexterity and practice may be requiſite in the obſerver, in managing the ſet ſcrew, ſo as to keep the object interſected by the wire; but if fine ſmooth ſcrews, ſuch as are uſed for micrometers to aſtronomical quadrants, are adapted to the inſtrument, as well that commanding the horizontal motion as the vertical, I apprehend the management will be perfectly eaſy and familiar to an obſerver, otherwiſe well practiſed.

It is eaſy to ſee, that thoſe ſtars are to be preferred that are neareſt the heavenly body to be obſerved; and that *cæteris paribus*, thoſe in ſuch poſitions, as riſe or fall ſlowly, are beſt for determining their altitude; and thoſe that alter their azimuth ſlowly, are beſt for determining the azimuth. To avoid intricacy in deſcription, I have ſuppoſed only two wires, interſecting each other at a right angle, in the focus of the teleſcope: but for the ſake of getting a medium in ſuch parts of the obſervation as depend on time, it will be proper to have not only three perpendicular wires, parallel to each other as common, but alſo three horizontal wires; the proportional diſtances of which being previouſly determined by obſervation, the oblique motions may, (in parts not near the pole) be conſidered as right lines.

This method is the more valuable, as it is entirely free from the knowledge of refractions; for, ſince the computation gives the real altitude from the time given, independent of refractions, and ſince the heavenly body is equally affected by refraction, at the ſame altitude, the computed altitude of the ſtar will give the real altitude of the heavenly body cleared of refraction, which never enters the queſtion; and, ſince ſuch ſtars may be choſen as will render the time intercepted ſhort, there is the leſs chance of a change of refraction, during the time, between the middle and laſt obſervation; and therefore this method will be particularly uſeful in obſervations near the horizon.

Austhorpe, April 17, 1768. J. SMEATON.

N. B. Obſervations of this kind may be made upon the planets in the day light, by making uſe of the ſun for the firſt obſervation inſtead of a ſtar; and waiting afterwards for the appearance of the ſtars.

OBSERVATION of a Solar Eclipse, the 4th of June, 1769, at the Observatory at Austhorpe, near Leeds, in the County of York, by J. SMEATON, F. R. S.

Read November 16, 1769.

	h.	′	″
Beginning by mean time, A. M. - - -	6	33	1
Middle - - - - - -	7	26	38
End - - - - - -	8	20	16
Total duration - - - - -	1	47	15
Digits eclipſed - - - - -	6	46	0

N. B. The beginning and end of the eclipſe were obſerved by an excellent $3\frac{1}{2}$ feet treble object glaſs teleſcope, conſtructed by DOLLAND, with the ſmalleſt magnifier, which enlarged the diameter ſomewhat above 80 times. As there is no defect in quantity of light from the ſun, the object glaſs was contracted by an aperture to $2\frac{1}{2}$ inches, and the object was perfectly ſharp and diſtinct.

The quantity was taken by a parallel wire micrometer, upon an equatorial apparatus, which rendered it very commodious for the purpoſe; by which the part of the ſun's diameter, remaining uneclipſed, meaſured at right angles to a line joining the horns, was 889, ſuch parts as the ſun's diameter, taken the ſame day at $1\frac{1}{2}$ hour in the afternoon, meaſured between two parallels of declination, 2041.

The latitude I have not yet got ſo correctly as I expected to do; but I do not at preſent know, whether it exceeds or falls ſhort of 53° 48′. The ſuppoſed longitude is 6′ of time weſt of Greenwich; this is deduced from its poſition with Wakefield, whoſe longitude is ſet down in "Maſkelyne's Britiſh Mariner's Guide," as determined from an obſervation of the tranſit of Venus of 1761.

The exact knowledge at what point of the ſun's circumference to look for the beginning (which was communicated to me by Mr. MASKELYNE), I found of great uſe, inſomuch that, I believe, I ſaw the firſt diſcernable impreſſion; I have, however, allowed 2″ for the time elapſed between the firſt perception, and the being ſure it was the approach of the moon that affected that part of the ſun's limb, and which latter only

only could be noted by the clock. The firſt approach did not, however, affect the ſun's circumference by any thing like a penumbra or ſhade, but began by ſome aſperities of the moon's limb, ſeeming to thurſt themſelves into that of the ſun; and which appeared before any continued part of the ſun's circumference was cut off, or, perhaps, it might be occaſioned by the firſt approach of the moon's limb, diſturbing the little protuberances upon the ſun's circumference, occaſioned by the undulation of the air, and which, when rendered exceedingly diſtinct, appeared almoſt like the teeth of a fine ſaw. This whole appearance, to a teleſcope leſs diſtinct, would probably look like a penumbra or ſhadow.

Some time before the great ſpot was immerged, there appeared two parts of the moon's circumference more protuberent than the reſt, near the right hand horn, which ſo remarkably interrupted the regulation of the curve, that it was taken notice of by all about me, and which, doubtleſs, was occaſioned by two mountains upon the moon's ſurface, remarkably higher than the reſt, and I doubt but the ſame thing will have occurred to other obſervers.

N. B. Mr. SMEATON was prevented, by clouds, from obſerving the entrance of Venus upon the ſun the evening before.

ACCOUNT

ACCOUNT of an Observation of the right Ascension and Declination of Mercury out of the Meridian, near his greatest Elongation, September, 1786, made by Mr. JOHN SMEATON, F. R. S. with an equatorial Micrometer, of his own Invention and Workmanship; accompanied with an Investigation of a Method of allowing for Refraction in such Kind of Observations.

Read at the Royal Society, June 7th, 1787.

M. DE LA LANDE having announced to ſome of my aſtronomical friends the utility of accurate obſervations of Mercury, at his two elongations the laſt year, in Auguſt and September; I tried to get obſervations of that planet in croſſing the meridian, for ſome days before and after the greateſt elongation in Auguſt; and though the ſtate of the atmoſphere about that time was not very favourable to the purpoſe, yet there was one day that I thought unexceptionable, but could not perceive the leaſt appearance of Mercury; at which I was the rather ſurpriſed, as I had formerly ſeen that planet in the like ſituation with the ſame inſtrument, with perfect perſpicuity *: and as I did not hear of any one elſe having ſucceeded in this obſervation, I thought it might be very poſſible for the ſame diſappointment again to happen, with reſpect to the approaching elongation in September. I judged, therefore, that it might be of ſome utility to aſtronomy, if, by *any means*, a good obſervation of Mercury could be got; and alſo, that it would be a proper ſubject whereon to make trial of an inſtrument for ſuch purpoſes, the idea of which I had conceived, and begun to conſtruct, above forty years before; but which,

* The inſtrument mentioned is a tranſit, made by myſelf in the year 1768; at which time achromatic object-glaſſes not having been, ſo far as I knew, applied to aſtronomical inſtruments by others, but having found the good effects thereof for other purpoſes, I reſolved to uſe a double achromatic object-glaſs, made by Mr. DOLLAND; which being of equal aperture with the ſimple object-glaſs, then in the tranſit of the Royal Obſervatory, this teleſcope I therefore eſteemed to be of nearly equal validity, as to quantity of light, with that at Greenwich, but reduced to the more commodious length of three feet ſix inches.

from

from various avocations, I did not perfect to my ſatisfaction till the year 1770 *: ſince which time it has lain by, in hopes that ſomething might happen, by which a full and effectual trial might be made thereof.

This inſtrument was originally intended as an improvement of the common wire micrometer, for the purpoſes of taking differences of right aſcenſion and declination, in a more commodious and effectual manner than could be done in the method then practiſed of uſing that inſtrument †; and at the ſame time more effectually to anſwer the purpoſe of GRAHAM's *aſtronomical ſector*, which was contrived by him (as Dr. SMITH informs us) to ſupply the deficiencies of the micrometer then in uſe ‡.

The moſt neceſſary and fundamental improvements ſeemed to be; firſt, that of rendering the micrometer teleſcope manageable upon an equatorial motion; and, ſecondly, the contrivance of a ſtand of ſuch ſolidity and ſtedfaſtneſs that the teleſcope might preſerve the poſition in which it was placed, for a length of time, for it occurred to me, that if the teleſcope could be maintained at reſt, or in a degree of ſtability ſuperior to that of the aſtronomical ſector, then the neceſſity of taking in a greater compaſs in declination than could be commodiouſly given to the field of a teleſcope would be the leſs neceſſary: for, inſtead of confining the object to a compariſon with a ſtar not differing more than a few minutes of time, or at moſt a quarter of an hour in right aſcenſion,

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* Some obſervations made therewith, after it was completed, I transmitted to my friend Mr. AUBERT; the conſiſtency of which induced him to procure a ſimilar inſtrument to be made for *cometary* and ſuch kind of obſervations as cannot be commodiouſly made in the meridian. Of this inſtrument the Rev. Mr. WOLLASTON has made honorable mention in Phil. Trans. Vol. LXXV. for 1785, p. 348.

† The common wire micrometer, as uſed by Dr. BRADLEY, and deſcribed from a paper in the Doctor's hand-writing, is given by Dr. MASKELYNE in the Philoſophical Tranſactions, Vol. LXII. for the year 1772; and in addition to which I muſt beg leave to obſerve, that the teleſcopes then in uſe for the micrometer were from ten to fifteen feet long, made with wooden tubes, ſupported at each end upon two wooden ſupports, by which the teleſcope could be managed in altitude and azimuth; but not to follow a celeſtial object in its proper motion on one centre; which apparatus, I believe, is ſtill remaining in the Royal Obſervatory.

‡ This inſtrument is deſcribed in SMITH's Optics, Vol. II. p. 350; and the original one, made by Mr. GRAHAM, was, at his death, placed in the Royal Obſervatory, and is mounted upon a three-legged ſtand of *wood*.

thoſe

thoſe compariſons could be extended to an hour or two, or even on occaſion to three or four hours; there being ſcarcely any part of the heavens ſo devoid of ſtars, of a ſuitable magnitude for theſe obſervations, but that a proper one may be found within that compaſs in right aſcenſion, provided there is allowed the difference of a degree, either north or ſouth of the object, in declination *.

Conſidering, however, that the approaching elongation would be in the morning; and that the beſt chance of ſeeing Mercury with this inſtrument would be ſome time in the twilight, between Mercury's riſing and the riſing of the ſun; yet, on ſuppoſition of catching the planet in his paſſage over the wires, there would be no chance of ſeeing any ſtar paſs over the field, wherewith to compare him, till the following evening, which, being at leaſt fourteen hours, the certain poſition of the teleſcope for ſo great a length of time was *almost* more than I could reaſonably hope for. To judge how far I might form an expectation, by way of a previous trial, I compared Saturn with γ Capricorni †, and found the return of the ſtar to the ſame place two evenings afterwards, both in right aſcenſion and declination, was ſo near, that I concluded I might very well expect a good obſervation of Mercury, in caſe I could get a ſight of him, though the ſtars wherewith he was to be compared lay at the diſtance of the following evening at the ſooneſt.

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* The field of the teleſcope of Mr. AUBERT's inſtrument is two degrees; but that of the original, wherewith this obſervation was made, is 1° 17′: to gain which, the eye-glaſs being immoveable (and *achromatic* to prevent the indiſtinctneſs that would otherwiſe have taken place near the border) the magnifying power was obliged to be conſiderably reduced, in reſpect of what has been uſual for micrometers, that is, ſo as not to exceed 20 times: in conſequence, there is, therefore, no need for ſo long a teleſcope, this being but 34$\frac{2}{3}$ inches focal length of object-glaſs; but being a double achromatic, made by the late Mr. JOHN DOLLAND, it is capable of as great an aperture as could be given to the ſimple object-glaſſes of twelve or fifteen feet teleſcopes, that were then generally given to micrometers; but the pencil of light being greateſt in this, it is attended with this advantage, that the ſmall ſtars can be ſeen very diſtinct and in great abundance, like the modern night-glaſſes: and there is in reality no need of great magnifying powers for the preſent purpoſe; for the place of the wire being viewed by an eye-glaſs, of about 1$\frac{1}{4}$ inches focus, its place may be diſtinguiſhed to leſs than a 2300th part of an inch, which, on the radius of 34$\frac{2}{3}$ inches, is ſcarce 2″$\frac{1}{2}$ of a degree; and which, as I apprehend, is nearer the truth than can reaſonably be expected from inſtruments out of the meridian.

† According to this obſervation ♄ preceded γ ♑ the 2d of September, at 9 h. 15′ P. M. mean time by 30′ 9″.7 MT, and with greater declination ſouth than γ by 41′ 23″.

The

The micrometer is furniſhed with five horary wires, denominated in their order *a*, A, B, C, D, (B being the middle horary wire) and the two declination wires are denominated *A* and *B*, each moveable by a ſeparate and independent micrometer-ſcrew, from the outſide of the field to the centre, and a little beyond it; ſo that each wire can be moved into the place of the other when at or near the centre *.

The morning of the 23d of September, about a quarter paſt five o'clock, the air being clear and perfectly ſerene, it being then about an hour after Mercury's riſing, and near three-quarters of an hour before the riſing of the ſun, I very readily found Mercury with the teleſcope, and when found could eaſily ſee him with an opera glaſs; and Mercury being then in a ſtate of very little alteration of declination, I adjuſted one of the declination wires to his apparent run, by making him traverſe the *whole* field. The obſervations were then taken as in the firſt table; and in the evening I was lucky enough to get thoſe of λ Ceti and *o* Tauri, intending to repeat the whole the next morning and evening. The next morning proved cloudy, and ſo continued, that I ſaw the planet no more; but in the evening of the 26th, I found the ſtars come again ſo near the ſame declination, that I was encouraged to continue the obſervation to ſee what change would happen. It then came on bad rainy weather till the 30th, when I again repeated the obſervation, and found the ſtars to come ſo near in declination, that I was fully ſatisfied of the ſtability of the inſtrument, ſo far at leaſt as could regard twenty-four hours: but as I was then appointed to go a journey, and could have no other uſe for the inſtrument, I locked the door of the Obſervatory, leaving the inſtrument in its poſition, that I might ſee what change would happen by the time of my return; and was quite aſtoniſhed to find, on the 13th of October, that it had remained in a manner unmoved; for it had ſuffered no more apparent alteration than what might occur by the errors of obſerving, and alterations of the clocks and tranſit.

* In Dr. BRADLEY's Paper it is ſaid, that before the *late* alterations, both the declination wires were made moveable; and that it was an improvement to make one of them fixed, and one only moveable. But however they might be immediately preceding the Doctor's time, I believe the original micrometers by Mr. TOWNLEY were with one fixed and one moveable declination wire, as I have ſeen one in this form among the remaining apparatus of Mr. ABRAHAM SHARPE. In an inſtrument, however, fitted up for the purpoſes of the *equatorial micrometer*, I believe it will be found moſt convenient to have both thoſe wires moveable; as by this means they not only are enabled to ſlide into *each other's place*, but every part of the frame of the inſtrument remains fixed during the whole of the obſervation, the two ſlides carrying theſe two wires excepted.

It

It muſt however be remarked, that, beſides that in the conſtruction of the inſtrument, every thing was contrived that appeared likely to give it firmneſs; it was reſted upon the *frustum* of an hexagonal pyramid of ſtone, in the founding whereof, great care was taken as to its ſolidity; and was detached from the floor for ſupporting the obſerver.

This Obſervatory at Auſthorpe I eſteem in the latitude of 53° 47′ 54″ N. and 5′ 50″ of time W. from Greenwich.

TABLE

MERCANTILE LIBRARY NEW-YORK

TABLE I.

Obſervations of Mercury at his Elongation, Sept. 1786, with an Equatorial Micrometer.

Day, Object, and wires.	Hour	Time as taken by the clock	Time reduced to min. & sec.	Reduced to the middle wire	Mean of the wires	Parts of the microm.	Micrometer reduc.
Sept. 23.	A M.	M. q. bea.	′ ″	′ ″	′ ″	Rev. Pts.	Rev. Pts.
Merc. to wire *a*	5	24 8 5	24 47.5	26 34.8			
A	—	25 3 14½	25 52.3	26 34.8			
Middle wire B	—	26 2 9½	26 34.7	26 34.7	26 34.7	*B* 28.85	S 0.74
C	—	27 1 6	27 18	26 34.7	N. B. The telescope's centre was pointed to horary circle VI. 34½ Decl. N. 7° 48′.		
D	—	28 1 15	28 22.5	26 34.6			
	P M.						
λ Ceti to B	9	15 1 27	15 28.5	15 28.5	15 28.4		
C	—	16 0 23	16 11.5	15 28.3			
o Tauri to B	—	40 1 25	40 27.5	40 27.5	40 27.4	*B* 8. 39	N 19.72
C	—	41 0 21	41 10.5	40 27.3			
Sept. 26.							
λ Ceti to *a*	9	2 2 13	2 36.5	4 23.5			
A	—	3 2 23	3 41.5	4 23.9	4 23.8	*B* 16.97	N 11.14
C	—	5 0 14½	5 7.3	4 24.1			
o Tauri to A	—	28 2 21	28 40.5	29 22.9			
B	—	29 1 16	29 23	29 23	29 23	*B* 8.47	N 19.64
Sept. 30.							
λ Ceti to *a*	8	47 0 3½	47 1.8	48 48.8			
A	—	48 0 13	48 6.5	48 48.9			
B	—	48 3 8	48 49	48 49	48 49	*B* 16.97	N 11.14
C	—	49 2 4½	49 32.3	48 49.1			
o Tauri to A	9	13 0 11	13 5.5	13 47.9			
B	—	13 3 6	13 48	13 48	13 48.1	*B* 8.48	N 19.63
C	—	14 2 3	14 31.5	13 48.3			
a Orionis to *a*	11	41 3 12	41 51	43 38			
A	—	42 3 20½	42 53.3	43 37.7			
B	—	43 2 17	43 38.5	43 38.5	43 37.9	*A* 15.07	S 15.77
C	—	44 1 12	44 21	43 37.8			
D	—	45 1 20½	45 23	43 37.7			
Oct. 13.							
λ Ceti to A	7	58 3 0	58 45	59 27.4			
B	—	59 1 25½	59 27.7	59 27.7	59 27.5	*B* 16.97	N 11.14
C	8	0 0 21½	59 10.7	59 27.5			
o Tauri to C	—	25 0 20	25 10	24 26.8	24 26.8	*B* 8.50	N 19.61
a Orionis to *a*	10	52 2 0½	52 30.2	54 17.2			
A	—	53 2 9	53 34.5	54 16.9			
B	—	54 1 4½	54 17.2	54 17.2	54 17.1	*A* 15.07	S 15.77
C	—	55 0 0½	55 0.2	54 17			
D	—	56 0 10	56 5	54 17.4			
1	2	3	4	5	6	7	8

TABLE II.

For reducing the horary Wires of the Equatorial Micrometer to that of the Middle, when taken in mean Solar Time.

	Wires.	Equatorial Object.		Declination 7° 48′.	
		☉'s run	☼'s run	☉'s run	☼'s run
The 1ſt wire precedes the middle, add	*a*	′ ″ 1 46.2	′ ″ 1 46	′ ″ 1 47.3	′ ″ 1 47
2d ———	A	1 42.1	1 42	1 42.5	1 42.4
3d, or middle wire — —	B	— —	— —	— —	— —
4th is ſubſequent to the middle, ſubtract	C	1 42.9	1 42.8	1 43.3	1 43.2
5th ———	D	1 46.8	1 46.6	1 47.9	1 47.6
1	2	3	4		6

TABLE

TABLE III.

Containing the Obſervations of Table I. reduced ſo as to ſhew the correct Differences of right Aſcenſion and Declination between Mercury and the Stars wherewith he was compared.

1786. Date and object.	Hour.	Paſſage over mid. hor. wire by journ. clock.	Correction to reduce the clock to mean time.	Correct mean time of the obſervation.	Intervals of mean time of different obſervations.	Parts of micrometer from the teleſcope's centre.	Pts. of micr. reduced = declin. from the teleſcope's centre.
Sept. 23.	A M	′ ″	′ ″	h. ′ ″		Rev. Pts.	′ ″
☿ to the mid. wire	5	26 34.7	—3 59.8	5 22 34.9		S 0.74	S 1 8
	P M				h. ′ ″		
λ Ceti to mid. wire	9	15 28.4	—4 0.1	9 11 28.3	15 48 53.4		
o Tauri to the ſame	9	40 27.4	—4 0.1	9 36 27.3	0 24 59	N 19.72	N 30 26
Sept. 26.							
λ Ceti to mid. wire	9	4 23.8	—4 43.2	8 59 40.6		N 11.14	N 17 11
o Tauri to the ſame	9	29 23	—4 43.2	9 24 39.8	0 24 59.2	N 19.64	N 30 18
Sept. 30.							
λ Ceti to mid. wire	8	48 49	—4 50.9	8 43 58.1		N 11.14	N 17 11
o Tauri to the ſame	9	13 48.1	—4 50.9	9 8 57.2	0 24 59.1	N 19.63	N 30 17
α Orion. to the ſame	11	43 37.9	—4 50.8	11 38 47.1	2 29 49.9	S 15.77	S 20 20
Oct. 13.							
λ Ceti to mid. wire	7	59 27.5	—6 36.4	7 52 51.3		N 11.14	N 17 11
o Tauri to the ſame	8	24 26.8	—6 36.4	8 17 50	0 24 58.7	N 19.61	N 30 15
α Orion. to the ſame	10	54 17.1	—6 37	10 47 40.1	2 29 50.1	S 15.77	S 20 20
1	2	3	4	5	6	7	8

TABLE IV.

Deviations in the Direction of the Axis of the Teleſcope of the Equatorial Micrometer, in right Aſcenſion and Declination in twenty Days, from the 23d of September to the 13th of October, both incluſive.

Objects obſerved.		3 days from the 23d to 26th.	4 days from the 26th to 30th.	7 days from the 23d to 30th.	13 days from the 30th to 13th.	17 days from the 26th to 13th.	20 days from the 23d to 13th.
λ Ceti	R. Aſcenſion	" 0	too late 1.1	too late 1.1	too ſoon 0.1	too late 1.0	too late 1.1
	Declination	not taken the 23d	exact 0	not taken the 23d	exact 0	exact 0.	not taken the 23d.
o Tauri	R. Aſcenſion	too late 0.2	too late 1.0	too late 1.2	too ſoon 0.5	too late 0.5	too late 0.7
	Declination	South 8.	South 1.	South 9.	North 2.	North 3.	South 11.
α Orionis	R. Aſcenſion	— — —	— —	— — —	too ſoon 0.3	— — —	— — —
	Declination	— — —	— —	— — —	South 5.	— — —	— — —

N. B. The right aſcenſion is expreſſed in the integer and decimal parts of a ſecond of time.

The declination is expreſſed in ſeconds of a degree.

Explanation of the less obvious Parts of the Tables of the Observation of Mercury *near his Elongation,* Sept. 1786.

The third column of Table I. contains the times of obſervation as they were taken down from the half-ſecond *journeyman* clock, in minutes, quarters, and *beats*, according to the following method; which was, by taking up the beat when the ſecond hand came to 15, 30, 45, or 60, and then counting 30 beats repeatedly till the arrival of the object at the middle of the wire it was approaching; after its arrival, the beats (or interval between two beats) being retained in memory, and the eye caſt upon the dial-plate, it was eaſily ſeen whether it was ſo many beats more than the quarter, the half, three-quarters, or the whole minute, and was ſet down accordingly. Thoſe reduced to minutes, ſeconds, and tenths of ſeconds, by allowing .3 for the quarter ſecond, .5 the half, and .7 for the three-quarters of a ſecond, are contained in the fourth column. The reduction of the fourth column to the fifth was by means of the auxiliary Table II.; and Mercury being then nearly ſtationary reſpecting the ſun, the ſun's run was uſed for the planet inſtead of that of a ſtar. The mean of each ſet of obſervations of the fifth column are carried into the ſixth.

The ſeventh column contains the parts of the micrometer as they were read off; to render which intelligible, it is to be noted, that the declination wire *A* travels from the upper ſide of the field of view of the teleſcope towards the centre, and ſomewhat beyond it; and upon it are taken all the objects that paſs the field of view on the upper ſide, anſwerable (by inverſion of the object) to the ſouthern half of the field: and in like manner thoſe that paſs the field of view on the lower half are taken upon the wire *B*, and for the ſame reaſon denote a declination north. The ſcale of the micrometers of each wire begins from a point aſſumed ſomewhat without the field, and the number increaſes from thence towards the centre of the field, and continues beyond it; the integral parts are the turns of the ſcrew, and the centeſimal the diviſions of the index plate, being divided into 100 parts. The point of the ſcale, anſwerable to the centre of the field of view, having been found by obſervations on each ſcale reſpectively; when the wire *A (Auſtralis)* ſtands at 30.84, it is in the centre of the field; and when the wire *B (Borealis)* is at 28.11, it alſo cuts the ſame centre. Hence the parts of the micrometer being reſpectively taken from thoſe two numbers (which may therefore be called *conſtant* numbers) the remainder will be the diſtance of each reſpective wire from the centre in parts of the micrometer. Thus, in the obſervation of *o* Tauri upon the 23d, the

the parts are *B* 8.39; this taken from 28.11, leaves N 19.72, which are placed in col. 8. as the diſtance, in parts of the micrometer, that *o* Tauri paſſed north of the centre of the field of view, or axis of the teleſcope.

In like manner, in the obſervation of Mercury on the 23d, the parts are *B* 28.85; but this being greater than the conſtant number 28.11, the exceſs will be .74 parts; which being the parts reaching beyond the centre, they will be ſo much *south* of it, and are ſet down therefore in col. 8. S 0.74: and in this manner the declinations of the reſt are made out, from their reſpective numbers of parts of the micrometer, and ſet down in col. 8.

The numbers of the ſixth column of Table I. are transferred to the third column of Table III.; and the declinations ſet down in parts of the micrometer, Table I. col. 8, are transferred to col. 7. of Table III.

Col. 4. of this table, contains the corrections of the times deduced from the journeyman clock (as *per* col. 3.) to reduce it to mean time; which corrections are made out from the general account of the goings of the tranſit clock, corrected by tranſits of the ſun, taken the 22d, 23d, 27th, and 30th of September, and the 12th, 13th, and 14th of October *. The journeyman clock was regularly compared at nights and mornings with the tranſit clock; and generally immediately after the obſervation. The *meridian* and *rotative* obſervatories in which the clock reſpectively were, are at the diſtance of 53 yards E. and W.; the compariſons were made by a ſeconds ſtop watch †.

The numbers of the fourth column being properly applied to thoſe of the third produce the fifth; and which, with the ſixth column, will be ſufficiently explained by their titles. The parts of the micrometer in the ſeventh column, being reduced into minutes and ſeconds, are contained in col. 8. and reſpectively ſhew the minutes and ſeconds at which each object paſſed to the north or ſouth of the centre of the teleſcope. The value of the parts of the micrometer were obtained by previous obſervations, from

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* The transit clock was made by HINDLEY, and has a pendulum rod of *cedar* wood.

† The journeyman clock was generally set to the transit clock on Sunday mornings; and when from home the former was suffered to go down. The journeyman will *generally* agree with the transit clock to 2" in 24 hours; but during the period of these observations, went remarkably well.

whence

whence the following rule was deduced: the numbers of turns and centeſimal parts being conſidered as integral, and divided by 1.08, the quotient will be the number of ſeconds. Thus, in the obſervation of *o* Tauri upon the 23d, the parts 1972, divided by 1.08, gives $1826'' = 30' 26''$; and the parts of Mercury .74, divided by 1.08 = $68'' = 1' 8''$. Now the teleſcope being fixed to one point of the heavens during the whole period of theſe obſervations, without any motion of any of the parts, the ſcrews commanding the declination wires *A* and *B* excepted, we are enabled to judge of its ſteadineſs to this point by the following remarks. If it varied in declination, this would be ſhewn by the paſſage of the ſame ſtar at a different diſtance from the centre of the teleſcope at different revolutions; and if it varied in right aſcenſion, it would be ſhewn by its not paſſing the horary wires at the due time, according to the acceleration of the ſtars upon the mean time of the ſun. Both the right aſcenſion and declination may be varied by differences of refraction of the air at the ſame altitude; and the right aſcenſion is further liable to be *apparently* varied, by the errors of the tranſit inſtrument, the tranſit clock, the transferring of its time to the journeyman clock, the intermediate errors of the ſame, and of the obſervation itſelf; and as there paſſed an interval of almoſt 16 hours betwixt the paſſage of Mercury over the field of view of the teleſcope and that of λ Ceti, which was the neareſt ſtar wherewith a compariſon could be made, it will be a ſatisfaction to ſee, as before intimated, what variations aroſe in ſtill greater intervals of time.

In right aſcenſion.

	h.	′	″
Thus λ Ceti upon Sept. 23, paſſed the horary wires at	9	11	28.3
and ——— 26, ———	8	59	40.6
λ Ceti therefore came ſooner in three days by		11	47.7
but ——— ought to accelerate on mean time		11	47.7
——— therefore came after three days exactly to the time			
Again, *o* Tauri upon Sept. 23, paſſed the horary wires at	9	36	27.3
and ——— 26, ———	9	24	39.8
o Tauri therefore came ſooner after three days by		11	47.5
——— ought to accelerate on mean time		11	47.7
——— therefore came too late in three days by			.2

In

In declination.

	′	″
o Tauri upon Sept. 23, passed north of telescope's centre - - -	30	26
——— 26, ——— - - -	30	18
——— therefore passed less north, or more south, than before by - -		8

In like manner every compariſon that Table III. affords is particularly ſet down in Table IV. which, containing thirteen compariſons in right aſcenſion and ten in declination, the greateſt deviation in right aſcenſion is 1″.2, and 11″ of a degree in declination. This ſuppoſes every error before mentioned to reſide in the inſtrument, and every other inſtrument and obſervation, which were concerned in the reſult, to be perfect; which, from the ſmallneſs of the total errors, ſeems to indicate a degree of ſteadineſs in the inſtrument unexperienced or unnoticed before.

Deduction of the Position of Mercury from the preceding Observations as set down in Table III.

In right aſcenſion from column 6.

	′	″
o Tauri followed λ Ceti Sept. 23. - - - - - -	24	59
——— 26. - - - - - -	24	59.2
——— 30. - - - - - -	24	59.1
——— Oct. 13. - - - - - -	24	58.7
——— at a mean of the four - - - -	24	59

	h.	′	″
α Orionis followed o Tauri Sept. 30. - - - - -	2	29	49.9
——— Oct. 13, - - - - -	2	29	50.1
——— at a mean - - - - -	2	29	50

Now Mercury preceded λ Ceti Sept. 23, - - - - -	15	48	53.4
λ Ceti preceded o Tauri by mean of four - - - - -		24	59
o Tauri preceded α Orionis by mean of two - - - -	2	29	50
Mercury therefore preceded α Orionis by - - - - -	18	43	42.4

In

In declination from column 8.

Sept. 23. A. M. Mercury passed the middle horary wire, south of its centre				′ 1	″ 8
Same evening o Tauri passed the middle horary wire, north of it				30	26
Therefore Mercury passed the middle horary wire more south than o Tauri by				31	34

But Sept. 26, λ Ceti passed north of centre	17	11	Diff.	13	7
—— o Tauri ——	30	18			
—— 30, λ Ceti ——	17	11	——	13	6
—— o Tauri ——	30	17			
—— Oct. 13, λ Ceti ——	17	11	——	13	4
—— o Tauri ——	30	15			

From the ſmallneſs of the above differences we may infer, that very little uncertainty in declination had attended the paſſage of o Tauri upon Sept. 23.

Upon Sept. 30, o Tauri passed north	30	17	Sum	54	37
—— α Orionis —— ſouth	24	20			
Upon Oct. 13, o Tauri passed north	30	15	——	54	35
—— α Orionis —— ſouth	24	20			

α Orionis then at a mean paſſed more ſouth than o Tauri	′ 54	″ 36
Mercury therefore on the 23d paſſed with more north declination that α Orionis	23	2

Inveſtigation of the Effects of Refraction.

The preceding deductions and remarks ſhew the conſiſtency of the obſervations with themſelves; yet, from the poſition of the teleſcope, it being only elevated 11°½ above the horizon *, it is neceſſary to examine how far the deductions above ſpecified were capable of being affected by refraction. And in this reſpect it will appear, that

* This will readily be deduced by inspection of the celestial globe.

if it be ſuppoſed, there is no difference in the quantity of refraction of ſuch objects as appear within the limits of the field of view of this inſtrument (which is 1° 17′), then their relative poſitions to each other will not be affected thereby: for if in fig. 1. Plate IX. we ſuppoſe the circle VHRO to repreſent the boundary of the field of view, HO being an horizontal and VR a vertical line, each paſſing through the centre of the field at L; and if PLP denotes a part of a parallel of declination, then BLX perpendicular thereto, will be a part of an horary circle, both paſſing through the ſame centre. Now let d ✳ be the apparent path of a ſtar, ſuppoſing it unaffected by refraction till it comes to the vertical line at ✳, and there to be lifted up by refraction in the ſaid vertical to L. Let e + denote another ſtar, alſo unaffected by refraction, to paſs along the different parallel of declination e + till it comes to +; then, if it be ſuppoſed that the two ſtars are both ſituated in the ſame horary circle, if at the point + refraction takes place, and by hypotheſis this is lifted up equally with the other, in the perpendicular + l, then the line +✳ being drawn through the places of the two ſtars, will be cotemporary and parallel to LX; and the figure l +✳L being evidently a rhomboides, the two ſtars, ſo altered by refraction, will arrive together at the horary circle LX at the ſame time, and with the ſame difference of declination, as if no refraction had taken place. It is therefore only the *difference* of refraction which takes place in objects at different heights in the *ſame field*, that can alter their relative ſituations: however, it appears neceſſary to examine what this may amount to.

Let the letters in fig. 2, denote the ſame things as before; to which we will add, that a, A, B, C, D, denote the parallel horary wires of the micrometer, and AA, BB, the declination wires, denoted A and B in the tables: now from the celeſtial globe we ſhall alſo readily obtain the horary angle VLI = 54°½=Lbc. Let now an object paſs along the wire AA from the horizontal line at d to the vertical line at b; in this it will paſs through a difference of refraction, according as it gets more and more elevated above the horizontal line HO; and let the elevation Lb be half a degree or 30 minutes: then, according to Dr. BRADLEY'S Table of Refraction *, the difference of refraction betwixt the 78th and 79th degrees of zenith diſtance is 23″.6, half of which 11″.8, may be eſteemed the difference of refraction for a difference of half a degree of altitude at 78°½ zenith diſtance, or of 11°½ altitude: the

* Inſerted in Dr. MASKELYNE'S Obſervations, Vol. I. p. 15.

object,

Fig: 2.

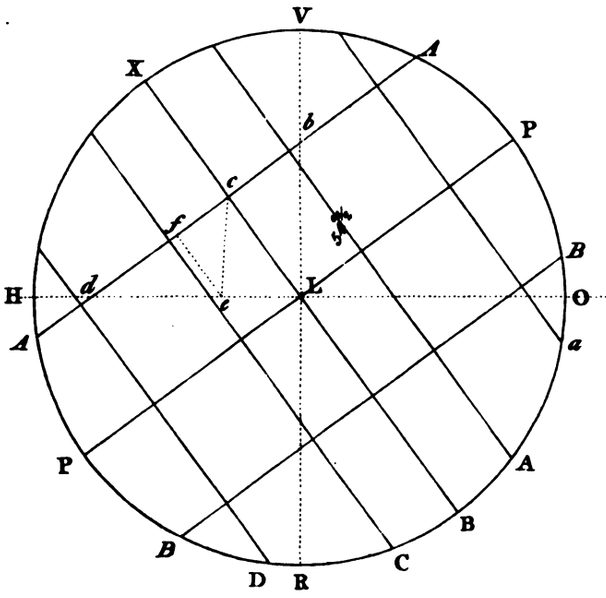
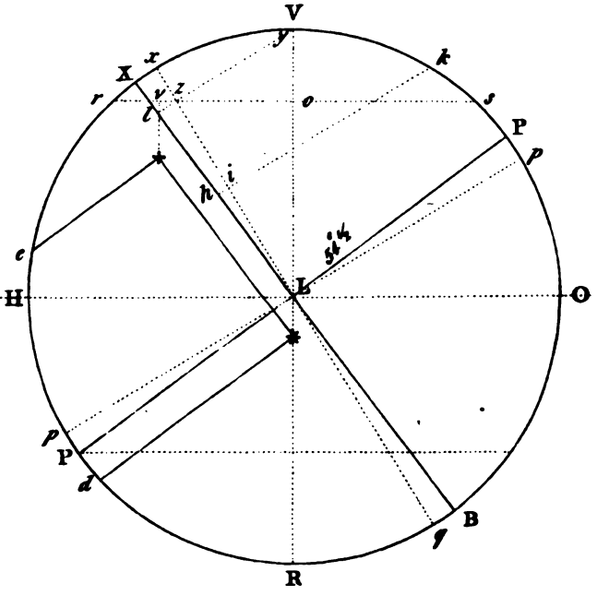
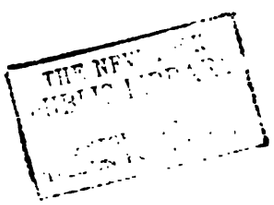


Fig: 1.





object, therefore, in paſſing from the horizontal line at *d* to the vertical line at *b* paſſes through every difference of refraction from 0″ to 11″.8; and the queſtion is, how much it is at a *medium*, that is, when it arrives at the middle wire at the point *c*? From this point let fall the perpendicular *ce*. Now, the proportion of the ſides of the triangle *db*L being given from conſtruction, they may be taken off by a ſcale viz.

Suppoſe L*b*=174
db=299
*d*L=242
and aſſuming the ſide L*b*=30
the other ſides by proportion as above { *db*=51.6
will be { *d*L=41.7

The triangles L*bc* and *dce* are ſimilar to *db*L; therefore ſay, as *db*=51.6 : *d*L=41.7 :: *Lb*=30 : L*c*=24; and as L*b*=30 : L*c*=24 :: *d*L=41.7 : *dc*=33.5; and again, as *db*=51.6 : *dc*=33.5 :: L*b*=30 : *ce*=19.5: but this will affect the declination, only in proportion of the line *ef* drawn parallel to LX; and it will affect the right aſcenſion according to the line *fc*: but the triangle *ecf* being ſimilar to the original one *db*L, we ſhall have *db*=51.6 : L*b*=30 :: *ce*=19.5 : *fc*=11.3 for the line affecting the right aſcenſion; and alſo, as *db*=51.6 : *d*L=41.7 :: *ce*=19.5 : *ef*=15.8 for the line affecting the declination. But the effect of difference of refraction upon the line L*b*=30′ being only 11″.8, the reſpective effects of the lines *fc* and *ef* will be in proportion; that is,

as 30′ : 11′.3 :: 11″.8 : 4″.4 for the effect in right aſcenſion,
and as 30 : 15.8 :: 11.8 : 6.2 ——————— declination;

but, as it has been determined, that when the line L*b* is 30 minutes, the line LC, or the correſponding declination, will be only 24 minutes; the effects of refraction above ſtated will be therefore due to 24′.

Correction for the Position of the Wires.

The above corrections take place on ſuppoſition that the ſeveral wires of the micrometer were ſtrictly parallel to the reſpective parts of the circles of declination, and horary circles in the heavens; but in the practical uſe of this inſtrument it is found more convenient, on account of a ready and certain adjuſtment, to place one of the wires

wires *AA* or *BB* parallel to the apparent track of the ſtar wherewith the *planetary* body is to be compared: in conſequence, when the ſtar ✳, fig. 1. is lifted up to L, it will not ſtrictly purſue the line LP; but being leſs and leſs lifted up as it mounts higher, it will apparently fall more and more below the line LP as it aſcends above the line HO, and will therefore take a courſe, ſuppoſe *Lp*. The wire PLP being therefore adjuſted to agree with *p*L*p*; by conſtruction of the inſtrument, the wire BLX will aſſume the poſition *q*L*x* perpendicular to *p*L*p*. The ſtar, therefore, that ran along the parallel *e*+before it ſuffered refraction, and at + was ſuppoſed to be lifted up to *l*, there not meeting LX will take the courſe *ly*, nearly parallel to L*p*, and have ſome diſtance, as *lz*, to travel before it arrives at the new-placed wire L*x*; and it is now proper to examine what this quantity may be.

Through the point *z* draw the line *r z o s* parallel to HO, and cutting the vertical RLV in *o*, and let L*o* be aſſumed=30′; then, ſince the angle XL*x* is ſuppoſed to be *minute*, the *groſs* proportions of the ſides of the triangles L*yz* and L*yl* may be, for this purpoſe, ſuppoſed the ſame *, and the ſame as L*bc*, *db*L, fig. 2. to which the triangle L*zo*, fig. 1. will alſo be ſimilar; as likewiſe the triangle *yzo*, and alſo the little triangle *zlv*: but making the ſide *lv* of the triangle *zlv* equal to the effect of refraction in perpendicular=11″.8; then, to find the ſide *lz*, the diſtance run from firſt to the laſt ſuppoſed place of the wire, we need only ſay, as L*b*=30 : *db*=51.6 :: *lv*=11″.8 : *lz*=20″.3; and this will be its value when the declination L*c*, fig. 2. is 24′; but then the declination L*l* or L*z*, fig. 1. being greater than the perpendicular ſide L*o* (aſſumed 30′) in the proportion of L*z* : L*a*, ſay, by ſimilarity of triangles converſely, as *d*L=41.7 : *db*=51.6 :: *Lo*=30′ : *Lz*=38′.2; but as the correction before ſtated of 20″.3 is an angular error, taking place in proportion to the diſtance from the centre, or the declination; for the declination given of 24′ ſay, as 38.2 : 24′ :: 20″.3 : 13; to which, adding 4″.4, we ſhall have 17″.4 for the whole error in right aſcenſion, ſuppoſing it in the equator, but muſt be again increaſed in the proportion in which a ſtar having declination is ſlower than a ſtar in the equator; that is, it muſt be increaſed in the proportion of any of the numbers in

* I am aware, that the ſuppoſition of the ſides of the triangles L*yz* and L*yl* being the ſame cannot be ſtrictly ſo; nor can they have the ſame proportions; nor are any of the lines concerned right lines, that are ſuppoſed ſuch; but aſſumptions *near* the truth are allowable for the correction of an error in the *greateſt part*, that if uncorrected would ſcarcely amount to a *groſs error*.

the

the fourth column of Table II. to the ſimilar ones in col. 6. of the ſame table; that is, as 1′ 46″ : 1′ 47″ or as 106 : 107, :: 17″.4 : 17″6 *.

As all theſe errors, ariſing from difference of refraction, are in proportion of the diſtance of the object from the centre of the teleſcope, they will take place in proportion to the difference of declination of the two objects to be compared, whether they have paſſed the field on the ſame or on different ſides of the centre. Now the difference of declination of Mercury and α Orionis being only 23′ 2″, and the quantities being made out for 24′ ſay (rejecting the 2 ſeconds), as 24′ : 23′ :: 17″.4 : 16″.7, which turned into time in the run of the ſtar will be 1″.1 in right aſcenſion.

Say again, as 24′ : 23′ :: 6″.2 : 6″, the correction in declination. From the near equality of the lines L*l*′ and L*z*, it is evident, that no correction of *declination* is neceſſary on account of the inclination of the wires, the whole difference falling in right aſcenſion. As therefore Mercury paſſed with 23′ 2″ more north declination than α Orionis, and paſſed through a part of the *medium* that lifted him up leſs; it therefore gave him leſs north declination than it did to α, and therefore apparently diminiſhed the real difference; hence 6″ muſt be added to the apparent difference 23′ 2″, making it 23′ 8″, difference of declination: and as Mercury was lifted up leſs than α, he would not ſo ſoon come to the middle wire by 1″.1 as he ſhould have done, he therefore came too late by 1″.1, which muſt be ſubtracted from the time of Mercury's paſſage the 2d of September, which will increaſe the time in which he preceded α Orionis; that is, 18 h. 43′ 42″.4 increaſed by 1.1 will become 18 h. 43′ 43″.5 difference of right aſcenſion.

I have been the more particular in the inveſtigation of this obſervation, firſt of all to aſcertain the degree of dependance that may be formed on an inſtrument of the kind; and, ſecondly, to infer ſuch eaſy and ſimple rules, that other ſimilar obſervations may be the more eaſily reduced. Being therefore ſatisfied of the ſtability of the inſtrument; if we had concluded the obſervation with that of Mercury in the morning, and of *o* Tauri in the evening of the 23d, then the reſult from Table III. ſhould have been

	h.	′	″
Mercury paſſed the wires at - - - - - -	17	22	34.9
And *o* Tauri paſſed at - - - - - -	9	36	27.3
Difference of right aſcenſion - - - - - -	15	13	52.4

* My friend Dr. MASKELYNE obſerves, that in *ſtrictneſs* each ſtar ought to have its own proper reduction, on account of *difference* of declination, which in *extreme cases* will amount to a ſenſible quantity.

which

which is the very ſame as was before deduced from the mean of the whole:

	′	″
And if to Mercury's declination ſouth of teleſcope's centre - - -	1	8
We add *o* Tauri's —— north —— - - -	30	26
We ſhall have for the difference of declination - - - -	31	34

the ſame as before determined. Our obſervation would, therefore, in this caſe ſimply have been, that Mercury preceded *o* Tauri in right aſcenſion 15h. 13′ 52″.4 mean time, and paſſed the wire with more ſouth declination than *o* Tauri by 31′ 34″.

After this, *o* Tauri would have required to be compared with ſome well rectified ſtar by meridian inſtruments; but in the preſent caſe α Orionis, one of Dr. MASKEYLNE'S Catalogue of 34 principal ſtars happened to lie ſufficiently near the ſame parallel of declination, to admit of *o* Tauri to be compared therewith by the ſame inſtrument, while pointed to the ſame place of the heavens. The operations which were ſubſequent, therefore, muſt be conſidered as intended to ſave thoſe of a meridian inſtrument.

Now, had our obſervation concluded with the above, then the correction would have taken place upon the difference of declination of *o* Tauri with Mercury, inſtead of the ultimate one with α Orionis; but it muſt be obſerved, that whatever quantity of correction the difference of declination would occaſion, it would be compenſated in the difference of refraction of *o* Tauri and α Orionis, when they came to be obſerved on the meridian; however, in the preſent caſe it happens to be more commodious, as both can be done under one.

Preparatory then to the laying down the ſimple rule for the correction of refraction, it is proper to premiſe, that it is evident, the lines, fig. 2 L*b*, L*c*, *ce*, *ef*, being in continued proportion L*b* will be to *ef* in triplicate proportion of L*b* to L*c*; and that L*c* will be to *ef* in duplicate proportion of L*b* : L*c*. The difference of declination, therefore, due to 30′ difference of elevation will be as L*b* to L*c* ſimply; but the effect of difference of refraction in declination will be leſs than the difference of declination in the proportion of $Lb^2 : Lc^2$; and that the effect of difference of refraction in right aſcenſion will be leſs than the difference of refraction in declination in the proportion of L*b* : *cb* ſimply.

Now it has been remarked, that the elevation of the teleſcope's centre above the horizon, and the horary angle VLP, will always be readily given near enough for the purpoſe by the globe. A triangle given L*bd* can therefore be conſtructed, and the ſide

L*b*

L*b* being made 30′ (or any convenient aliquot part of a degree) the other ſides will be found by proportion: ſay then, as in the preſent caſe, *db*=51.6 : *d*L=41.7 :: L*b*=30 : L*c*=24, for the difference of declination correſponding to half a degree of altitude: ſay then, as $51.6^2 : 41.7^2$, that is, as 2663 : 1739 :: 24 : 15.7=*ef*. But without troubling ourſelves with high numbers, if we take the proportion, 51.6 to 41.7 by the ſlide-rule *twice*, we ſhall arrive at 15.7, near enough for the value of the line *ef*: ſay then, as L*b*=30 : *ef* 15.7 :: 11″.8 : 6″.2 for the refraction in declination: and as *d*L=41.7 : L*b* : 30 :: 6″.2 : 4″.4 for the refraction in right aſcenſion, according to the *true* poſition of the wires: and for the correction of right aſcenſion in the poſition of the wires, ſay,

Fig. 2. Fig. 1.

As L*b*=30 : *db*=51.6 :: *h*=11″.8 : *lz*=20″.3;
and again, *d*L=41.7 · *db*=51.6 :: L*o*=30′ : L*z*=38′.2.

Take now L*h*, fig. 1.=L*c*, fig. 2.=24′, and draw the line *hik*, fig. 1, parallel to the line *lzy*, and then ſay, as L*z*=38′.2 : L*h* (or L*i*)=24′ : : *lz*=20″.3 : *hi*=13″, which +4″.4=17″.4 for the whole error in right aſcenſion, with a declination or diſtance from the centre of 24′; but as the errors both of right aſcenſion and declination are in proportion to diſtance from the centre, as the difference of the planet and ſtar is only 23′, ſay, as 24′ : 23′ : : 17″.4 : 16″.7=1″.1 time; and for the declination, ſay again, as 24′ : 23′ : : 6″.2 : 6″ declination *.

Reduction of Mercury's Comparison with α Orionis to right Aſcenſion and Declination.

We have laid it down, that the 23d Sept. 1786, A. M. at 5 h. 22′ 34″.9 mean time, Mercury preceded α Orionis 18 h. 43′ 43″.5, and had then a more northern declination by 23′ 8″.

According to Dr. MASKELYNE's Catalogue of 34 ſtars, the right aſcenſion of α Orionis reduced to the time when he was obſerved is 85° 54′ 12″.

---

* If the comparison had been with o Tauri, then we must have said,

As 24′ : 31½′ : : 17″.5 : 23″=1″.5 of time,
and 24′ : 31½′ : : 6.2 : 8.1 correction for declination.

N. B. All these and the above proportions will be commodiously wrought with the slide-rule.

Now,

Now, as the whole circle of the ſphere makes a revolution in the time that α Orionis makes one turn, which is

h.	′	″	
23	56	4.1	then from this deduct
18	43	43.5	
5	12	20.6	remains for the time that α Orionis preceded Mercury in right aſcenſion;

but if α ran the whole rotation=360° in 23 h. 56′ 4″.1, what portion of it will be run in 5 h. 12′ 20″.6=18740″.6?

But 24 h.=86,400 ſeconds, and 360=1,296,000 ſeconds:

Time.	Time.	Of degrees. Of degrees.			
Say then, as 86400″ : 18740″.6 : : 1296000″ : 281109″=				78°	5′	9″
But, according to Dr. MASKELYNE's ſelect Catalogue, the right aſcenſion of α Orionis for Sept. 30, 1786, was (which add)				85	54	12
The right aſcenſion of Mercury at the time of obſervation was therefore				163	59	21
According to Dr. MASKELYNE's ſelect Catalogue α Orionis had declination north, corrected for preceſſion				7	21	8.8
The ſum of aberration and nutation from *Connoiſſance des Temps*					+	8.4
The correct declination north of α Orionis				7	21	17.2
To which add that Mercury paſſed more north					23	8
Mercury's declination therefore was				7	44	25.2

The Result.

1786, Sept. 23, A. M. at 5 h. 22′ 35″ M. T.	Mercury's	right aſcenſion	163	59	21
		declination north	7	44	25

DESCRIPTION

DESCRIPTION of an Improvement in the Application of the Quadrant of Altitude to a Celestial Globe, for the Resolution of Problems dependant on Azimuth and Altitude, by Mr. JOHN SMEATON, F. R. S.

Read at the Royal Society, November 20th, 1788.

PERHAPS there are few inſtruments that better fulfil their deſign in general, or more naturally repreſent the movements they are intended to explain and illuſtrate, than the terreſtrial and celeſtial globe, which are alſo applied to reſolve ſome of the problems of the ſphere, which they moſt readily do. I believe, however, that whoever applies to them for the laſt mentioned purpoſe, will find them more defective in ſome reſpects than they are in others.

The difficulty that has occurred in fixing a ſemicircle, ſo as to have a centre in the *zenith* and *nadir* points of the globe, at the ſame time that the meridian is left at liberty to raiſe the pole to its deſired elevation, I ſuppoſe, has induced the globe-makers to be contented with the *ſtrip* of thin flexible braſs, called the *quadrant of altitude*; and it is well known how imperfectly it performs its office.

The improvement I have attempted, is in the application of a *quadrant of altitude*, of a more ſolid conſtruction; which being affixed to a braſs ſocket of ſome length, and this ground, and made to turn upon an upright ſteel ſpindle, fixed in the zenith, ſteadily directs the *quadrant*, or rather *arc*, of *altitude* to its true *azimuth*, without being at liberty to deviate from a vertical circle to the right hand or left: by which means the azimuth and altitude are given with the ſame exactneſs as the meaſure of any other of the great circles.

With reſpect to the horary circle, as the common application ſeems very convenient on account of the ready adjuſtment of its index to anſwer the culmination of any of the heavenly bodies; and as I find that a circle of four inches diameter is capable of an actual and very diſtinguiſhable diviſion into 720 parts, anſwerable to two minutes of time each, which may ſerve a globe of the largeſt ſize; it ſeems that it ſhould rather be *improved* than omitted; and, if inſtead of a *pointer*, an index *ſtroke* is uſed in the ſame plane with that of the diviſions, the ſingle minutes, and even half minutes, may be readily diſtinguiſhed.

This globe, though mounted merely as a model for experiment, and only nine inches in diameter, appears capable of bringing out the ſolution to a quarter of a degree; which, I apprehend, may be eſteemed ſufficient not only as a check upon numerical computation, but to come near enough to find ſtars in the day-time in the field of teleſcopes, which, having no equatorial motion, are only capable of direction in altitude and azimuth; but from globes of a larger ſize, we may expect to come proportionably nearer.

Explanation of the Figures, Plate X.

The figures 1. and 2. being different views of the ſame things, AB repreſents a line, in common to both, in the ſurface of the horizon, which here is of braſs.

CD, CD, are vertical lines, ſuppoſed to paſs through the centre of the globe in each figure; and

EFG, EFG, are portions of great circles of the globe.

Fig. 1. ſuppoſes the ſpectator looking at the apparatus of the globe from the ſouth point of the horizon; therefore the circular arch EFG, in this poſition, will be a part of the *prime vertical*, and the ſmall paralellogram HI is ſuppoſed to be a *ſection* of the braſs meridian, according to that vertical plane.

Fig. 2. is a view of the ſame parts, the ſpectator being ſuppoſed to look at them from the weſt point of the horizon; and in this poſition HI is ſuppoſed to be a *portion* of the *braſs meridian*. This being fixed in mind, in what follows the ſame letters denote the ſame parts in both figures.—KLM denotes a piece of braſs, or braſs carriage, made to fit upon the vertical part of the meridian, and capable of ſliding 5° on each ſide of that point, ſo as adjuſt to it, and to fix faſt there, by means of the finger ſcrew N*. This piece of braſs carries the *ſteel ſpindle* PQ which is firmly ſocketed into it at K, according to the dotted lines *o r*, *o r*. The axis of this ſpindle is therefore capable of being ſet upright upon the *zenith point*, and to maintain that poſition with a ſufficient

* The holes represented in the portion of the brass meridian (HI, fig. 2.) are screw holes at five degrees distance, in this quarter of the circle, into any of which the finger screw N is to be put, as occasion may require; the *slit* allowing sufficiently for adjustment.

degree

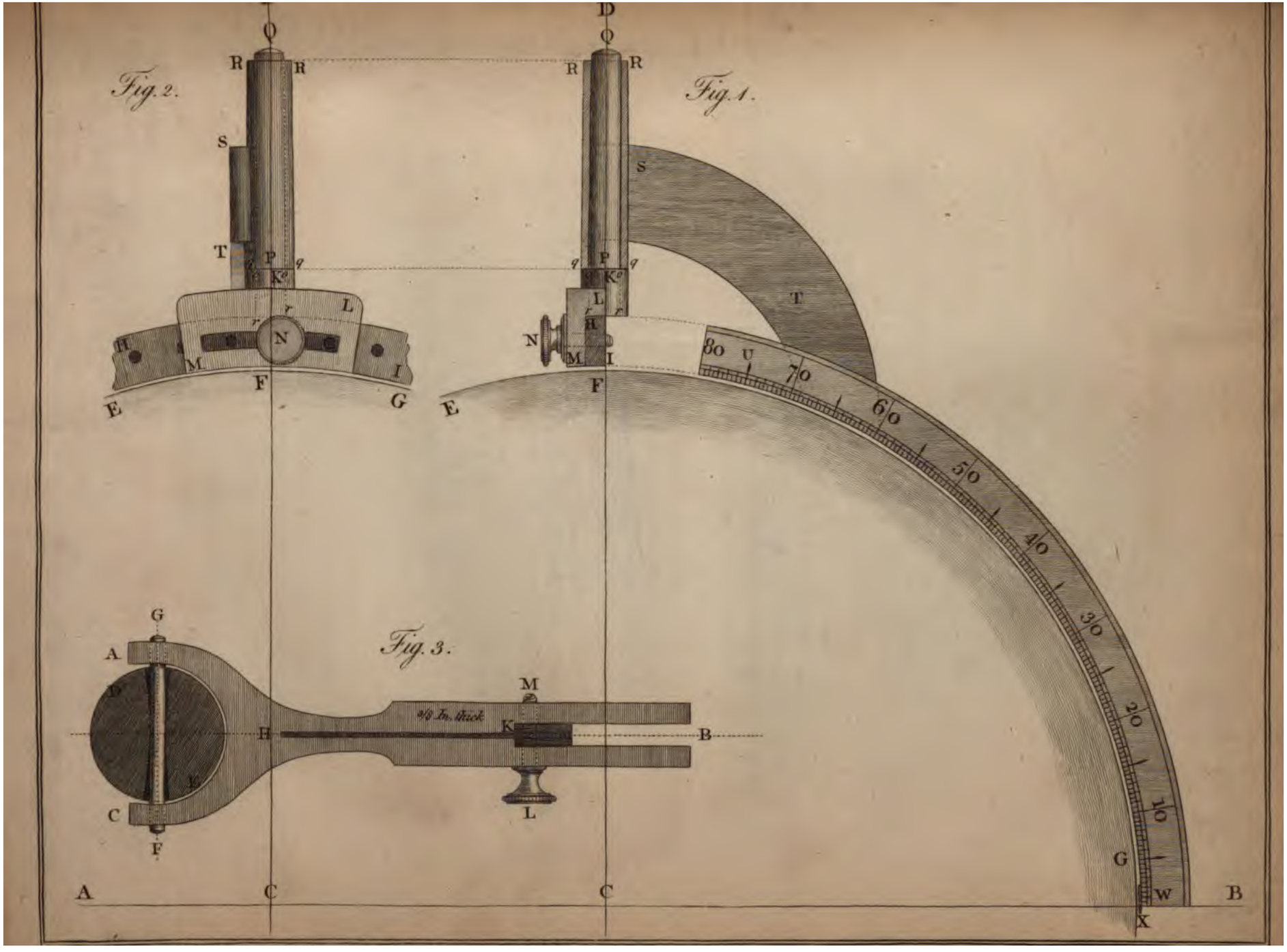


Fig. 2.
Fig. 1.
Fig. 3.
D
Q
R
R
S
T
P
q
K
L
r
N
M
F
E
G
I
80
U
70
60
50
40
30
20
10
G
W
X
B
A
C
H
K
B
M
L
C
F
E
D
A
1/2 In. thick

confining it in any other reſpect.—What I have found to anſwer this purpoſe is repreſented, fig. 3. The crutch-like piece of wood ABC is ſhewn as ſeen looking right down upon it. The circle DE is the horizontal ſection of the ſouth pillar of the globe. The ſtrong wire pin FG, that goes through the two arms of the crutch and pillar, ſerves as an axis upon which its other extremity at B is at free liberty to lift up and down, but without ſhake upon the pin; and the whole being ſplit with a fine ſaw, from B to H, the notch BK lays hold of the underſide of the braſs meridian, and by tightening the finger ſcrew LM, it firmly clips it, and retains it in any given poſition. And that it may be under no confinement *cross-ways*, the hole in the pillar is opened on both ſides, as ſhewn in the ſection, to give it liberty of accommodation; the pin being faſt in the two ends of the crutch, and turning gently in the pillar; the whole being ſlender and compliant, except in point of length.

N. B. Thoſe that would uſe the globe to the beſt advantage to ſolve problems, ſhould be careful to get a *just* declination, as alſo a *distinct* point to mark it; and as the circles and diviſions upon the ſurface of the globe itſelf, are not always ſufficiently to be depended on for this purpoſe, I have found the following expedient fully to anſwer. Chooſe any plain white part of the globe's ſurface, anſwerable to the declination given, and with the point of a needle or protracting pin, by the help of the diviſions of the braſs meridian, mark a fine point upon the blank ſurface of the globe, and upon this point make a dot with ink, with the ſmall point of a pen, which rub off with the finger, and it will leave a fine black ſpeck behind. This dot being brought to the meridian, rectify the horary index to it, and it will accurately repreſent the centre of the celeſtial body whoſe inveſtigation is wanted.

A DESCRIPTION

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Fig. 1. p. 608.

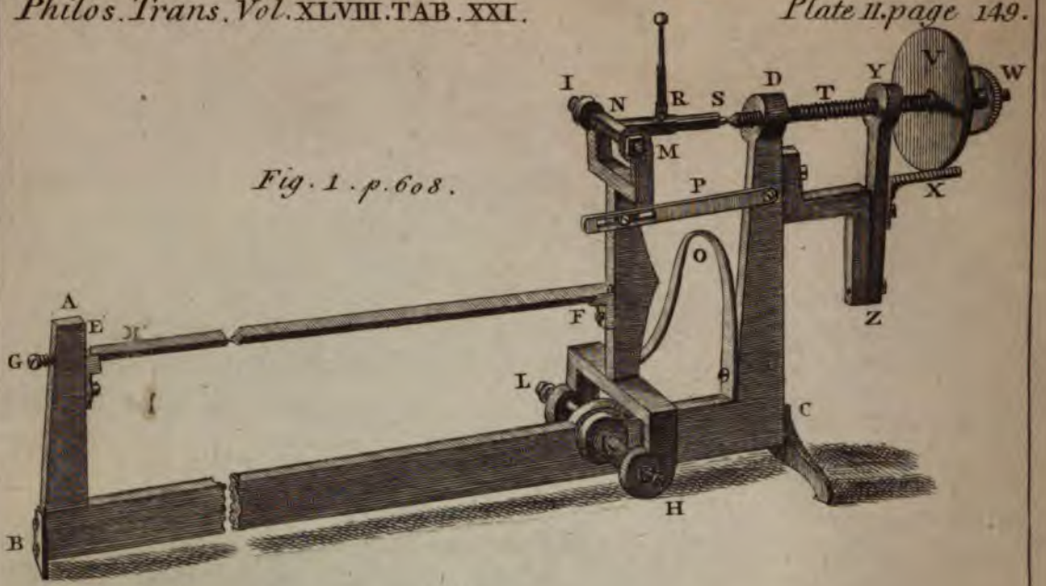
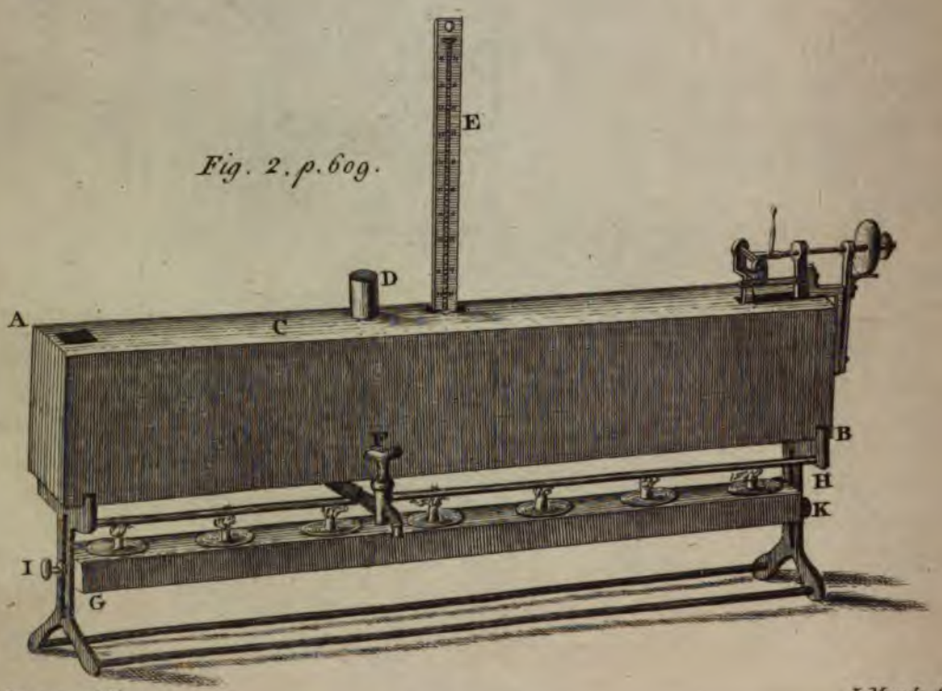


Fig. 2. p. 609.



J. Smeaton delin. *J. Mynde sculp.*

A DESCRIPTION of a new Pyrometer, with a Table of Experiments made therewith, by J. SMEATON, F. R. S.

See PLATE XI.

AS it may tend to illuſtrate the following account, it may not be improper to make mention of the properties, that a complete inſtrument, for meaſuring the expanſions that metalline bodies are ſubject to by heat and cold, ought to be endowed with.

And, firſt, ſince the quantities of thoſe expanſions muſt be proportionable to the length of the bar to be meaſured; the longer the bar, the more ſenſible the expanſion: And therefore ſuch a conſtruction is beſt, as *(cæteris paribus)* will admit of the longeſt bar.

Secondly, That the ſcale, whereupon thoſe minute alterations are to be meaſured, ought to be, at leaſt, ſo large, that the ſmalleſt change in the length of the bar, which the inſtrument is capable of being with certainty affected with, ought to be perceivable thereon.

Thirdly, As the ſame change, with reſpect to the poſition of the index and ſcale, will enſue, upon the ſuppoſition, that the materials, compoſing the inſtrument itſelf, are expanded in a certain degree, and the bar applied to be meaſured remains unchanged in its length; as if the inſtrument were ſuppoſed to ſuffer no expanſion, and the bar to be meaſured were ſuppoſed to expand in the former degree: It is therefore neceſſary, that, in the making uſe of an inſtrument of this kind, the materials, of which thoſe parts are compoſed, upon which the meaſure depends, and which may be called the baſis thereof, ſhould be ſubject to no expanſion or contraction during ſuch trial, or that the expanſion or contraction thereof ſhould be capable of being known, and accounted for.

Fourthly, That as all bodies grow ſtill longer by the application of a greater degree of heat; to compare the expanſions of different bodies, we ought to have ſome method of heating them in the ſame degree, notwithſtanding their difference of texture, ſpecific gravity, &c.

Fifthly, The ſeveral parts, upon which the meaſurements depend, ought to be ſufficiently large, to be themſelves actually meaſured; that not only the proportions of increaſe of length in different metals, by the ſame degrees of heat, may be known; but alſo

alſo the quantities of thoſe expanſions, in real meaſures: Or, in other words, the proportions, that their increaſe of length, between certain degrees of heat, bear to the length of the bodies: By which means, we are enabled to aſcertain the changes that bodies undergo in their dimenſions by the application of any given degrees of heat.

With reſpect to the firſt property, this inſtrument is capable of receiving a bar two feet four inches long, and might be made capable of receiving bars of a much greater length, of ſome kinds of materials, but not of others; on account of the flexibility brought upon them by a degree of heat not greater than boiling water.

The meaſures taken by this inſtrument are determined by the contact of a piece of metal with the point of a micrometer-ſcrew. The obſervation is the beſt judged of by the hearing, rather than that of the ſight or feeling. By this method I have found it very practicable, to repeat the ſame meaſurement ſeveral times, without differing from itſelf above one twenty-thouſandth part of an inch. This principle of determining meaſures by contact is not wholly new; but has been employed on ſeveral occaſions, as I am informed, by the late Mr. Graham: But the preſent manner of applying thereof, I believe, is ſo; and the degree of ſenſibility ariſing therefrom exceeds any thing I have met with. As the method will eaſily appear by the draught, I ſhall avoid a farther deſcription of it in this place *.

As no ſubſtance has hitherto been diſcovered in nature, that is perfectly free from expanſion by heat, I choſe to conſtruct this inſtrument in ſuch a manner, that the bar, which makes the baſis of the inſtrument, ſhall in each experiment ſuffer the ſame degree of heat, as the bar to be meaſured: Of conſequence, the meaſures taken by the micrometer are the differences of their expanſion. The expanſion then of the baſis between two given degrees of heat being once found, the abſolute expanſion of any other body, by adding or ſubtracting the difference to or from the expanſion of the baſis, according as the body to be meaſured expands more or leſs than the baſis, will alſo be determined.

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* I have lately ſeen an inſtrument at Mr. Short's, made by the late Mr. Graham, for meaſuring the minute alterations, in length, of metal bars; which were determined by advancing the point of a micrometer-ſcrew, till it ſenſibly ſtopped againſt the end of the bar to be meaſured. This ſcrew being ſmall, and very lightly hung, was capable of agreement within the 3 or 4000th part of an inch.

When

When the inſtrument is made uſe of, it is immerged, together with the bar to be meaſured, in a ciſtern of water; which water, by means of lamps applied underneath, is made to receive any intended degree of heat, not greater than that of boiling, and thereby communicates the ſame degree of heat to the inſtrument, the bar, and to a mercurial thermometer immerged therein, for the purpoſe of aſcertaining that degree. That this may be truly the caſe, the water ſhould be frequently ſtirred, that there may be no difference of heat in the different parts of the water: This being done, the height of the quickſilver appearing ſtationary, the contact with the ſcrew of the micrometer alſo remaining the ſame, for a ſpace of time, it is to be ſuppoſed, that the heat of the three bodies will be the ſame, as the heat of the water, however different they may be in ſpecific gravity, &c. The whole difficulty is now reduced to this problem, viz.

To find the abſolute expanſion of the baſis between any two given degrees of heat, not greater than that of boiling water.

For this purpoſe, let there be prepared a bar of ſtrait-grained white deal, or cedar; which, it is well known, are much leſs expanſible by heat than any metal hitherto diſcovered: Let the bar be adapted to the inſtrument in like manner as the other bars intended to be meaſured; but that the ſoftneſs of the wood may not hinder the juſtneſs of its bearings, let its ends be guarded with a bit of braſs let into the wood at the points of contact, to prevent, as much as may be, the moiſture or ſteam of the water from affecting the wood; let it firſt be well varniſhed, and then, being wrapped round with coarſe flax from end to end; this will, in a great meaſure, imbibe the vapour, before it arrives at the wood. Let the ciſtern alſo be ſo contrived, that the inſtrument being ſupported at a proper height therein, the bar to be meaſured may, upon occaſion, be above the cover, while the baſis remains in the water: Thus will the cover alſo be a defence againſt the moiſture. Let the water in the ciſtern be now brought to its lower degree of heat (ſuppoſe at or near the freezing point), the baſis having continued long enough in the water to receive the ſame degree of heat, and the wooden bar having been previouſly kept in an adjacent room, not ſubject to ſudden alterations of temperature by fire, or other cauſes; let the bar be applied to the inſtrument, and the degrees of the micrometer and the thermometer read off, and ſet down: Let the wooden bar be then reſtored to its former place, till the water is heated to the greater degree intended (ſuppoſe at or near that of boiling water); the lid being now ſhut down, and the chinks ſtopped with coarſe flax, to prevent the iſſuing of the ſteam as much as poſſible, let the wooden bar be again brought forth applied to the inſtrument, and the degrees of the micrometer

micrometer and thermometer read off, as before: The difference of degrees of the micrometer, correſponding to the difference of degrees of the thermometer, will expreſs the expanſion of the baſis between thoſe degrees of heat; that is, upon the ſuppoſition that the wooden bar was of the ſame length, at the time of taking the ſecond meaſure, as at the firſt: Indeed a meaſure can hardly be taken without any loſs of time, as the whole of the inſtrument, when the hot meaſure is to be taken, is conſiderably hotter than the wooden bar; and, in caſe of boiling water, the ſteam being very repellant and active, the bar is liable to be ſenſibly affected in its length, before the meaſure can be taken, both by heat and moiſture, which both tend to expand the bar: But as the quantity is ſmall, and capable of being nearly aſcertained, a wooden bar, thus applied, will anſwer the ſame end as if it was unalterable by heat or moiſture.

In order, therefore, to know the quantity of this alteration, let the time elapſed between the firſt approach of the bar to the inſtrument, and the taking of the meaſure, be obſerved by a ſecond-watch, or otherwiſe; after another equal interval of time, let a ſecond meaſure be taken; and after a third interval, a third; and a fourth; the three differences of theſe four meaſures will be found nearly to tally with three terms of a geometrical progreſſion, from which the preceding term may be known, and will be the correction; which, if applied to the meaſure firſt taken, reduces it to what it would have been if the wooden bar had not expanded during the taking thereof.

From a few obſervations of this kind, carefully repeated, the expanſion of the baſis may be ſettled; and this once done, the making experiments upon other bars will become very eaſy and compendious.

The baſis of this inſtrument (as well as other parts thereof) is braſs. I choſe this ſubſtance rather than any other whoſe expanſion was greater or leſs, becauſe I found, from ſome groſs experiments previouſly made, that the expanſion of braſs was nearly a medium between thoſe bodies, which differ moſt in their expanſion: A conſiderable convenience ariſes from this circumſtance; becauſe as the meaſures, taken in common experiments, are their difference from braſs, the dependance upon the thermometer will be leſs, as theſe differences are leſs. This precaution I have found the more neceſſary, as the greateſt errors that experiments made with this inſtrument are ſubject to, ſeem to be chiefly owing to the thermometer, though that which I uſed was well graduated, and good in other reſpects; but this muſt neceſſarily happen, as the ſcale and ſenſibility of the

the micrometer, when thoſe metals were tried which differ moſt from the baſis, were greater than that of the thermometer.

The bar of braſs which compoſes the baſis is an inch broad by half an inch thick, and ſtands edgeways upwards; one end is continued of the ſame piece at right angles, to the height of three inches and an half, and makes a firm ſupport for the end of the bar to be experimented; and the other end acts upon the middle of a lever of the ſecond kind, whoſe fulcrum is in the baſis; therefore the motion of the extremity of the lever is double the difference between the expanſion of the bar, and the baſis. This upper part of the lever riſes above the lid of the ciſtern, ſo that it and the micrometer-ſcrew are at all times clear of the water. The top of the lever is furniſhed with an appendage which I call the *feeler*; it is the extremity of this piece which comes in contact with the micrometer-ſcrew. The conſtruction and application hereof will better appear from the draught than from many words. It hence appears, that, having the length of the lever from its fulcrum to the point of ſuſpenſion of the feeler, the diſtance between the fulcrum and the point of contact with the bar, the inches and parts that correſpond to a certain number of threads of the micrometer, and the number of diviſions in the circumference of the index-plate; the fraction of an inch expreſſed by one diviſion of the plate may be deduced: Thoſe meaſures are as follows.

	Inches.
From the fulcrum of the lever to the feeler - -	5.875
From the fulcrum to the plate of contact - -	2.895
Length of 70 threads of the ſcrew - - -	2.455
Diviſions in the circumference of the index-plate 100.	

Hence the value of one diviſion will be the $\frac{1}{3\,7\,8\,6\,3}$ part of an inch: But if the ſcrew be altered $\frac{1}{4}$ of one of theſe diviſions, when the contact between the ſcrew and feeler is well adjuſted, the difference of contact (if I may ſo call it) will be very perceivable to the ſlighteſt obſerver; and, conſequently, $\frac{1}{1\,5\,1\,4\,5}$ part of an inch is perceivable in this inſtrument.

There is one thing ſtill remains to be ſpoken of, and that is, the verification of the micrometer-ſcrew, which is the only part of this inſtrument that requires exactneſs in the execution; and how difficult theſe are to make, perfectly good, is well known to every perſon of experience in theſe matters; that is, that the threads of the ſcrew may not

only be equidiſtant, in different places, but that the threads ſhall be equally inclined to the axis in every part of the circumference.

As nearly the ſame part of the ſcrew is made uſe of in theſe experiments, the latter circumſtance is what principally needs enquiry. For this purpoſe, let a thin ſlip of ſteel, or other metal, be prepared, whoſe thickneſs is about $\frac{1}{4}$ of the diſtance of the threads: Let the edges of this thin plate be cut into ſuch a ſhape, as exactly to fit into the fixed notch in which one end of the bar is laid: Let a ſcrew paſs through the ſtandard of braſs, on which that notch is ſupported, in ſuch a manner, that the end of the bar to be meaſured that is fartheſt from the lever, may take its bearing againſt the point (or rather the ſmall hemiſpherical end) of this ſcrew: Let one of the braſs bars, uſed in the other experiments, be applied to the inſtrument, and a meaſure taken; then let the thin plate be put in between the end of the bar and the point of the ſcrew laſt mentioned, and again take the meaſure; but firſt obſerve, that the plate is put down to the notch, ſo that the ſame place of the plate may always agree with the point of the ſcrew, and, conſequently, no error may ariſe from a different thickneſs in different places of the plate: Obſerve alſo, that the whole comes to a true bearing; then advance the ſame ſcrew till the micrometer-ſcrew is puſhed backward $\frac{1}{4}$ of a revolution; again repeat the meaſure with and without the thin plate; again advance the former ſcrew, ſo as to make that of the micrometer recede another quarter of a turn, and repeat the meaſures with and without the thin plate. This method being purſued as far as neceſſary, it is evident, that, the thickneſs of the plate being always the ſame, if the difference of meaſures, taken with and without it, are not always the ſame in the different parts of a revolution of the micrometer-ſcrew, that this ſcrew is not equiangular; but from the differences of the meaſures correſponding to the thickneſs of the ſame plate, in the different parts of a revolution, the errors thereof may be nearly aſſigned. For greater certainty in this examination, leſt the heat of the obſerver's body ſhould affect the bar or inſtrument during the obſervation, let the whole be immerged in the ciſtern of water, which ought to ſtand a ſufficient time before the obſervation is begun, to acquire the ſame temper as the air, which alſo ought to be in a ſettled ſtate.

In this manner I examined ſuch threads of this ſcrew as were made uſe of in the following experiments, but did not find any material errors.

The reſult of the experiments made with this inſtrument agrees very well with the proportions of expanſion of ſeveral metals given by Mr. Ellicott; which were deduced from

from his pyrometer publiſhed in the *Philoſophical Transactions:* And, conſidering the very different conſtruction of the two inſtruments, they abundantly tend to confirm each other.

References to the Figures.

Fig. 1. repreſents the inſtrument independent of the ciſtern in which it is uſed.

A B G D, is the main bar or baſis of the inſtrument.

E F, is the bar to be meaſured, lying in two notches; one fixed to the upright ſtandard A B, the other to the principal lever H I. The end E of the bar E F, bears againſt the point of

G, a ſcrew of uſe in examining the micrometer-ſcrew.

The other end of the bar F bears againſt a ſmall ſpherically protuberant bit of hard metal fixed at the ſame height as G, in the principal lever H I.

K, is an arbor fixed in the baſis, which receives at each end the points of the ſcrews H L, upon which the lever H L turns, and ſerve as a fulcrum thereto.

O, is a ſlender ſpring, to keep the lever in a bearing ſtate againſt the bar; and

P, is a check, to prevent the lever from falling forward when the bar is taken out.

N, is the feeler, ſomething in the ſhape of a T, ſuſpended, and moveable up and down upon the points of the ſcrews I M; which, as well as L H, are ſo adjuſted, as to leave the motion free, but without ſhake.

Q R, is the handle of the feeler, moveable upon a looſe joint at R; ſo that, laying hold of it at Q, the feeler is moved up and down without being affected by the irregular preſſure of the hand.

The extremity S of the feeler is alſo furniſhed with a bit of protuberant hard metal, to render its contact with the point of the micrometer-ſcrew more perfect.

I, is

I, is the micrometer-ſcrew; V, is the divided index-plate, and W, a knob for the handle.

The micrometer-ſcrew paſſes through two ſolid ſcrewed holes at D and Y.

The piece Y Z is made a little ſpringy, and endeavours to pull the ſcrew backwards from the hole at D; of conſequence keeps the micrometer-ſcrew conſtantly bearing againſt its threads the ſame way, and thereby renders the motion thereof perfectly ſteady and gentle.

X, is the index, having diviſions upon it, anſwering to the turns of the ſcrew. This piece points out the diviſions of the plate, as the face of the plate points out the diviſors upon the index.

When the inſtrument is uſed, lay hold of the knob at Q with one hand, and, moving the feeler up and down, with the other move forward the ſcrew I, till its point comes in contact with the feeler; then will the plate and index V and X ſhew the turns, and parts.

Fig. 2. repreſents the inſtrument immerged in its ciſtern of water, ready for uſe.

A B, is the ciſtern; C, the cover; which, when the inſtrument fig. 1. is raiſed upon blocks, goes on between the bar E F and the baſis B C.

D, a handle to take off the cover when hot; E, the mercurial thermometer; F, the cock to let out the water.

G H, a hollow piece of tin, which ſupports ſeven ſpirit lamps, which are raiſed higher or lower by the ſcrews I and K, in order to give the water in the ciſtern a proper degree of heat.

A TABLE

A TABLE of EXPERIMENTS,

By which the Numbers in Tab. I. Col. 6. N° 1, 2, *and* 4, *were determined.*

Experiment 1. The time elapſed between approaching the bar to the inſtrument and taking the firſt meaſure, was half a minute: Therefore the intervals between taking the ſucceeding meaſures was half a minute alſo. The firſt meaſure was 208; the ſecond 214½; the third 216½; the fourth 217½. The differences of theſe are 6½, 2, and 1; which pretty well tallies with the three laſt terms of the following geometrical progreſſion whoſe common diviſor is 2.8; viz. 17.7 : 6.3 : : 2.25 : .8; therefore, as the meaſures increaſed from the firſt, the firſt meaſure being diminiſhed by the firſt term, viz. 208 — 17.7 = 190.3, will be the true meaſure of the bar at the firſt inſtant of its application, before it was expanded by the heat and moiſture about the inſtrument.

Exp. 2. The firſt meaſure was 221¼; the ſecond, 227; third, 230½; fourth, 232¾; whoſe differences are 5¾, 3½, and 2¼; agreeing with the three laſt terms of the following progreſſion, whoſe common diviſor is 1.6; viz. 9.2 : 5.8 : : 3.6 : 2.2; therefore 221.25 — 9.2 212.15.

Exp. 3. The firſt meaſure taken was 401; and at that degree of heat the wooden bar did not ſenſibly alter during two minutes.

Exp. 4. The firſt meaſure taken was 275½; ſecond, 278½; third, 280¾; and the fourth, 282¼: The differences are, 3, 2¼, 1½; agreeing with the three laſt terms of the following progreſſion, whoſe common diviſor is 1.43; viz. 4.4 : 3.1 : : 2.15 : 1.5; therefore 275.5 — 4.4 = 271.1, which is the firſt meaſure corrected.

Hence, as appears by Tab. I. N° 1, 2, 3, and 4, Column 9. theſe experiments being properly reduced, agree within one diviſion of the micrometer; and the expanſion of the baſis, at a medium, is 287½ parts thereof; correſponding to 166° of Farenheit's thermometer.

TABLE

TABLE II.

A TABLE of Expansions of Metals,

Shewing, how much a foot in length of each, grows longer by an increaſe of heat correſponding to 180 degrees of Farenheit's thermometer, or to the difference between freezing and boiling water, expreſſed in ſuch parts whereof the unit is equal to the 10,000th part of an inch.

1	White glaſs barometer-tube.	100
2	Martial regulus of antimony.	130
3	Bliſtered ſteel.	138
4	Hard ſteel.	147
5	Iron.	151
6	Biſmuth.	167
7	Copper hammered.	204
8	Copper 8 parts, mixed with tin 1.	218
9	Caſt braſs.	225
10	Braſs 16 parts, with tin 1.	229
11	Braſs wire.	232
12	Speculum metal.	232
13	Spelter ſolder, viz. braſs 2 parts, zink 1.	247
14	Fine pewter.	274
15	Grain tin.	298
16	Soft ſolder, viz. lead 2, tin 1.	301
17	Zink 8 parts, with tin 1, a little hammered.	323
18	Lead.	344
19	Zink or ſpelter.	353
20	Zink hammered half an inch per foot.	373

P. S. It is now ſeveral years ſince I firſt obſerved the very conſiderable expanſion of the ſemi-metallic ſubſtance called *zink*, *ſpelter*, or *tootanag*; and propoſed it as more fit for the purpoſe of making compound pendulums, and metalline thermometers,

ters, than braſs; as its expanſion ſeemed conſiderably greater, and its conſiſtence, when gently hammered, not much inferior. With the ſame view I have made trial of ſeveral other metallic compoſitions, beſides what is above ſet down; but they all proved much inferior to zink in expanſion, and moſt of them in conſiſtence.

It ſeems, that metals obſerve a quite different proportion of expanſion in a fluid, to what they do in a ſolid ſtate: for regulus of antimony ſeemed to ſhrink in fixing, after being melted, conſiderably more than zink.

DESCRIPTION of a new Hygrometer, by Mr. JOHN SMEATON, F. R. S.

Read March 21, 1771.

HAVING ſome years ago attempted to make an accurate and ſenſible hygrometer, by means of a hempen cord, of a very conſiderable length, I quickly found, that, though it was more than ſufficiently ſuſceptible of every change in the humidity of the atmoſphere, yet the cord was, upon the whole, in a continual ſtate of lengthening. Though this change was the greateſt at firſt, yet it did not appear probable that any given time would bring it to a certainty; and furthermore, it ſeemed, that, as the cord grew more determinate in mean length, the alteration by certain differences of moiſture grew leſs. Now as, conſidering wood, paper, catgut, &c. there did not appear to be a likelihood of finding any ſubſtance ſufficiently ſenſible of differences of moiſture, that would be unalterable under the ſame degrees thereof; this led me to conſider of a conſtruction which would readily admit of an adjuſtment; ſo that, though the cord whereby the inſtrument is actuated may be variable in itſelf, both as to abſolute length under given degrees of moiſture, yet that, on ſuppoſition of a material departure from its original ſcale, it might be readily reſtored thereto, and in conſequence, that any number of hygrometers, ſimilarly conſtructed, might, like thermometers, be capable of ſpeaking the ſame language.

The two points of heat, the more readily determinable in a thermometer, are points of freezing and boiling water. In like manner, to conſtruct hygrometers which ſhall be capable of agreement, it is neceſſary to eſtabliſh two different degrees of a moiſture which ſhall be as fixed in themſelves, and to which we can as readily and as often have recourſe to as poſſible. One point is given by making the ſubſtance perfectly wet, which ſeems ſufficiently determinable; the other, that of perfect dry, but which I do not apprehend to be attainable with the ſame preciſion. A readineſs to imbibe wet, ſo that the ſubſtance may be ſoon and fully ſaturated, and alſo a facility of parting with its moiſture on being expoſed to the fire to dry, at the ſame time that neither immerſion in water, nor a moderate expoſition to the warmth of the fire, ſhall injure its texture, are properties requiſite to the firſt mover of ſuch an hygrometer, that in a manner exclude all ſubſtances that I am acquainted with, beſides hempen and flaxen threads or cords, and what are compounded thereof.

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Plate 12. page 163.

Phil. Trans. Vol. LXI. Tab. VI. p. 201.

Fig. 2.

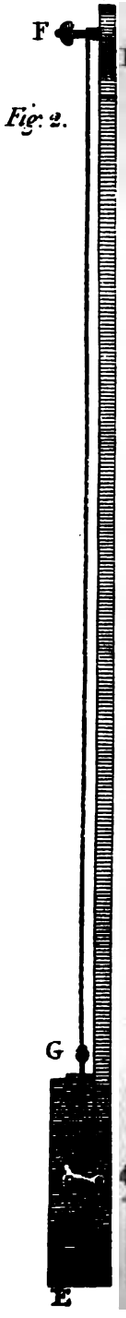


Fig. 3.

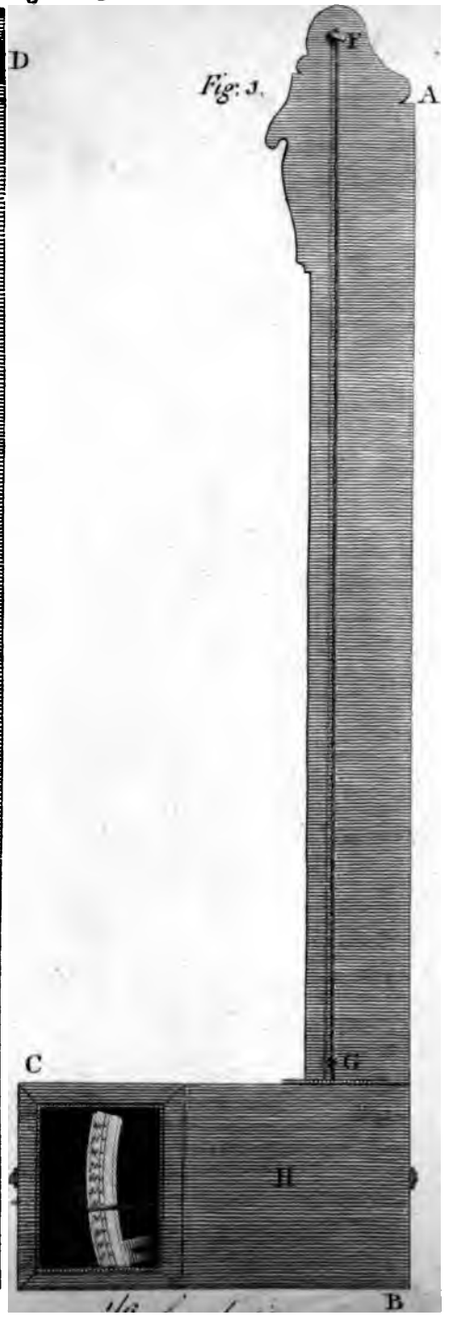


Fig. 3.

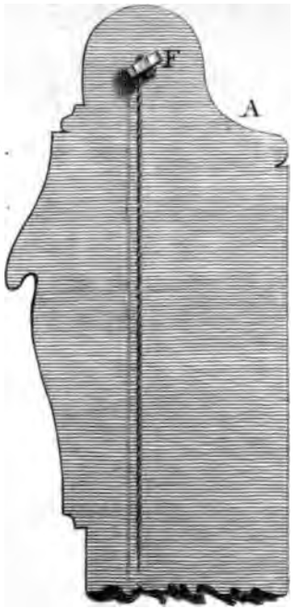
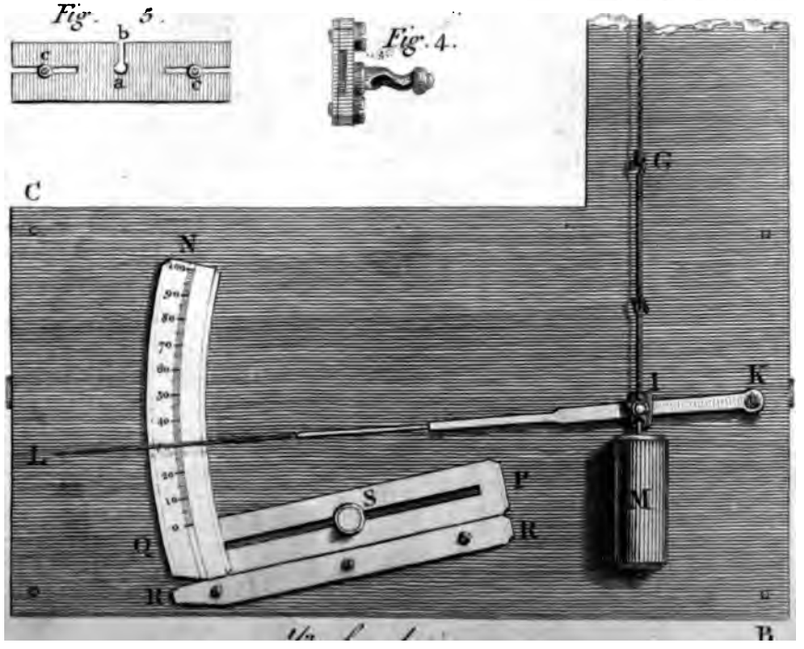


Fig. 5.

Fig. 4.



Upon theſe ideas, in the year 1758, I conſtructed two hygrometers, as near alike as poſſible, in order that I might have the means of examining their agreement or diſagreement on ſimilar or diſſimilar treatment. The interval or ſcale between dry and wet, I divided into 100 equal parts, which I call the degrees of this hygrometer. The point of *o* denotes perfect dry; and the numbers increaſe with the degrees of moiſture to 100, which denotes perfect wet.

On comparing them for ſome time, when hung up near together in a paſſage or ſtaircaſe, where they would be very little affected with fire, and where they would be expoſed to as free an air as poſſible in the inſide of the houſe, I found that they generally were within one degree, and very rarely differed two degrees; but, as theſe compariſons neceſſarily took up ſome time, and were frequently interrupted by long avocations from home, it was ſome years before I could form a tolerable judgment upon them. One thing I ſoon obſerved, not altogether to my liking, which was, that the flaxen cords, which I made uſe of, ſeemed to make ſo much reſiſtance to the entry of ſmall degrees of moiſture, (ſuch as is commonly experienced within doors in the ſituation above mentioned) that all the changes were compriſed within the firſt 30 degrees of the ſcale; but yet, on expoſing them to the warm ſteam of a waſh-houſe, the index quickly mounted to 100. I was therefore deſirous of impregnating the cords with ſomething of a ſaline nature, which ſhould diſpoſe them more forcibly to attract moiſture, in order that the index might, with the ordinary changes of moiſture in the atmoſphere, travel over a greater part of the ſcale of 100: how to do this in a regular and fixed quantity, was the ſubject of many experiments, and ſeveral years interrupted inquiry. At laſt, I tried the one hereafter deſcribed, which ſeemed to anſwer my intention in a great meaſure; and though, upon the whole, it does not appear likely that this inſtrument will ever be made capable of ſo accurate an agreement as mercurial thermometers are made to be, yet if we can reduce all the diſagreements of an hygrometer within $\frac{1}{100}$th part of the whole ſcale, it will probably be of uſe in ſome philoſophical inquiries, in lieu of inſtruments which have not been as yet reduced to any common ſcale at all.

Deſcription of the Hygrometer.

Plate XII.

Fig. 1 and 2, A B C, is an orthographic delineation of the whole inſtrument, ſeen in front in its true proportion. D E is that of the profile, or the inſtrument ſeen edgeways. F G, in both, repreſents a flaxen cord, about 35 inches long, ſuſpended by a turning peg F, and attached to a loop of braſs wire at A, which goes down into the box

box cover H, which defends the index, &c. from injury, and by a glaſs expoſes the ſcale to view.

Fig. 3 ſhews the inſtrument to a larger ſcale, the upright part being ſhortened, and the box cover removed; in which the ſame letters repreſent the ſame parts as in the preceding figures: G I are two loops or long links of braſs wire, which lay hold of the index K L, moveable upon a ſmall ſtud or centre K. The cord F G is kept moderately ſtrained by a weight M, of about half a pound avoirdupoiſe.

It is obvious, that as the cord lengthens and ſhortens, the extreme end of the index riſes and falls, and ſucceſſively paſſes over.

N Q, the ſcale, diſpoſed in the arch of a circle, and containing 100 equal diviſions. This ſcale is attached to the braſs ſliding ruler Q P, which moves upon the directing piece R R, fixed by ſcrews to the board, which makes the frame or baſe of the whole; and the ſcale and ruler N Q P is retained in any place, nearer to or further from the centre K, as may be required by the ſcrew S.

Fig. 4 repreſents, in profile, the ſliding piece, and ſtud I, (fig. 3) which traverſes upon that part of the index next the centre K, and which can, by the two ſcrews of the ſtud, be retained upon any part of the index that is made parallel, and which is done for 3 or 4 inches from the centre for that purpoſe. The ſtud is filed to the edges, like the fulcrum of a ſcale beam, one being formed on the underſide, the other upon the upper, and as near as may be to one another. An hook, formed at the lower end of the wire loops G I, retains the index by the lowermoſt edge of the ſtud, while the weight M hangs by a ſmall hook upon the upper edge; by theſe means the index is kept ſteady, and the cords ſtrained by the weight, with very little friction or burthen upon the central ſtud K.

Fig. 5 is a parallelogram of braſs plate, to keep out duſt, which is attached to the upper edge of the box cover H, and ſerves to ſhut the part of the box cover neceſſarily cut away, to give leave for the wire G I to traverſe with the ſliding ſtud (fig. 4) nearer to, or further from, the centre of the index K, and where in (fig. 5) *a* is a hole, about $\frac{1}{4}$th of an inch diameter, for the wire G I to paſs through, in the riſing and falling of the index, freely without touching; *b* is a ſlit of a leſſer ſize, ſufficient to paſs the wire, and admit the cover to come off without deranging the cord or index; *c c* are

two

two ſmall ſcrews applied to two ſlits, by which the plate ſlides lengthways, in order to adapt the hole *a* to the wire G I, at any place of the ſtud I upon the index K L.

Remarks on the preceding Conſtruction.

1ſt. In this conſtruction the index K L, being 12 inches long, 4 inches from the extreme end, is filed ſo narrow in the direction by which it is ſeen by the eye, that any part of theſe four inches, lying over the diviſions of the ſcale, becomes an index thereto. The ſcale itſelf ſlides 4 inches, ſo as to be brought under any part of the 4 inches of the index, attenuated as before mentioned.

2dly. The poſition of the directing piece R R is ſo determined, as to be parallel to a right line drawn through the point *o* upon the ſcale, and the centre K of the index; conſequently, as the attenuated part of the index forms a part of a radius, or right line from the ſame centre, it follows, that whenever the index points to *o* upon the ſcale, though the ſcale is moved nearer to or further from the centre of the index, yet it produces no change in the place to which the index points.

3dly. When the divided arch of the ſcale is at 10 inches from the centre (that is, at its mean diſtance), then the centre of the arch and the centre of the index are coincident. At other diſtances, the extremes of which are 8 or 12 inches, the centre of the diviſions and centre of the index pointing thereto, not being coincident, the index cannot move over ſpaces geometrically proportionable to one another in all ſituations of the ſcale; yet, the whole ſcale not exceeding 30 degrees of a circle, it will be found on computation that the error can never be ſo great as $\frac{1}{100}$th part of the ſcale, or 1 degree of the hygrometer; which in this inſtrument being conſidered as an indiviſible, the mechanical error will not be ſenſible.

Choice and Preparation of the Cord.

The cord here made uſe of is of flax, and betwixt $\frac{1}{150}$th and $\frac{1}{170}$th part of an inch in diameter, which can readily be aſcertained by meaſuring a number of turns made round a pencil or ſmall ſtick. It is a ſort of cord uſed in London for making nets, and is of that particular kind called by net-makers flaxen three threads laid. I do not imagine that the fabric of the cord is of the moſt material conſequence; but yet I ſuppoſe,

ſuppoſe, when cords can be had of ſimilar fabric, and nearly of the ſame ſize, that ſome ſmall ſcouces of variations will be avoided. In general, I look upon it that cords, the more they are twiſted, the more they vary by different degrees of moiſture, and the leſs we are certain of their abſolute length; therefore thoſe moderately twiſted, I ſuppoſe, are likely to anſwer beſt.

A competent quantity of this cord was boiled in one pound avoirdupoiſe of water, in which was put two pennyweights troy of common ſalt; the whole was reduced by boiling to ſix ounces avoirdupoiſe, which was done in about half an hour. As this aſcertains a given ſtrength of brine on taking out the cord, it may be ſuppoſed that every fibre of the cord is equally impregnated with ſalt. The cord being dried, it will be proper to ſtretch it, which may be done ſo as to prevent it from untwiſting, by tying three or four yards to two nails, againſt a wall, in an horizontal poſition, and hanging a weight of a pound or two to the middle, ſo as to make it form an obſtuſe angle. This done for a week or more in a room, will lay the fibres of the cord cloſe together, and prevent its ſtretching ſo faſt, after being applied to the inſtrument, as it otherwiſe would be apt to do.

I have mentioned the ſizes and principal dimenſions that I have uſed, as the inſtruments may as well be ſimilarly conſtructed as otherwiſe; but I do not apprehend it to be very material to agree in any thing but the ſtrength of the brine on taking the cord out of it. If the cord is adapted to the inſtrument ſome days before its firſt adjuſtment, I apprehend it will be the more ſettled.

Adjustment of the Hygrometer.

The box cover being taken off, to prevent its being ſpoiled by fire, and chooſing a day naturally dry, ſet the inſtrument nearly upright, about a yard from a moderate fire, ſo that the cord may become dry and the inſtrument warm, but not ſo near as would ſpoil the fineſt linen by too much heat, and yet fully evaporate the moiſture; there let the inſtrument ſtay till the index has got as low as it will go, now and then ſtroking the cord betwixt the thumb and finger downwards, in order to lay the fibres thereof cloſe together, and thereby cauſing it to lengthen as much as poſſible. When the index has thus become ſtationary, which will generally happen in about an hour (more or leſs as the air is naturally more or leſs dry), by means of the peg at top, raiſe or depreſs the index

index till it lays over point *o*; this done, remove the inſtrument from the fire, and having ready ſome warm water in a tea-cup, take a middling camel's-hair pencil, and dipping it in the water, gently anoint the cord till it will drink up no more, and till the index becomes ſtationary, and water will no more have effect upon it, which will generally happen in about an hour. If in this ſtate the index lays over the degree marked 100, all is right: if not, ſlack the ſcrew S, and ſlide the ſcale nearer to or further from the centre, till the point 100 comes under the index, and then the inſtrument is adjuſted for uſe; but, if the compaſs of the ſlide is not ſufficient to effect this, as may probably happen on the firſt adjuſtment, ſlack the proper ſcrews, and move the ſliding ſtud I nearer to or further from the centre of the index, according as the angle formed by the index, between points of wet and dry, happeneth to be too ſmall or too large for the ſcale; the quantity can be eaſily judged of, ſo as the next time to come within the compaſs of the ſlide of the ſcale, the quantity of ſlide being $\frac{1}{3}$d. of the length of the index, and conſequently its compaſs of adjuſtment $\frac{1}{3}$d of the whole variable quantity. Now, as ſliding the ſtud I will vary the poſition of the index reſpecting the point of *o*, this movement is only to be conſidered as a rough or preparatory adjuſtment, to bring it within the compaſs of the ſlide of the ſcale, which will not often happen to be neceſſary after the firſt time; but in this caſe the adjuſtment muſt be repeated in the ſame manner, by drying and wetting as before deſcribed.

It is to be remarked, that, as the cord is ſuppoſed to be impregnated in a given degree with common ſalt, and this not liable to evaporate, care muſt be taken in wetting that no drops of wet be ſuffered to fall from the cord: for, by the obſervance hereof, the original quantity is preſerved in the cord.

Obſervations made upon Two original Hygrometers.

Theſe hygrometers were firſt adjuſted, after the impregnation of the cords with common ſalt, in February, 1770; they were kept together in a ſtair-caſe till the ſummer following; they were frequently obſerved, and rarely found to differ more than one degree. In ſummer, one of them remaining in the former place, the other was removed into a paſſage through a building, which, having no doors, and the inſtrument being hung ſo that neither rain nor the direct rays of the ſun could fall upon it, thereby it became expoſed to the winds, and the free paſſage of the open air. In theſe ſituations the two hygrometers not only differed very greatly in quantity, but even frequently were moving different ways. They were thus continued till January, 1771, in which

ſpace

ſpace of time I obſerved, that the moſt ordinary place of the index was between 15° and 25° in the open air; that at 40° the atmoſphere felt very ſenſibly moiſt, but yet it was frequently above 60°, and more than once at 70°, or very near. I have therefore marked the point of *0* dry; 20° the mean, 40° moiſt, 70° very moiſt, 100° wet. I do not, however, mean thoſe words (thoſe of dry and wet excepted) with any other intent, than that of general direction, in like manner as thoſe upon the barometer, leaving the relative degree of moiſture to be judged of by the ſcale.

In the month of January laſt, I reſtored the expoſed hygrometer to its former place in the ſtair-caſe, when both inſtruments were again compared together, and they rarely differed more than 1°, and never ſo much as 2°. After this, they were both removed together to the out paſſage; and there they agreed nearly in the ſame manner, the utmoſt difference not exceeding 2°. After ſome trial here, one was re-adjuſted, leaving the other hanging in its place; on reſtoring the new adjuſted inſtrument to the other, they now differed about 5°, the new adjuſted one ſtanding ſo much higher. The day following the other was re-adjuſted alſo, and afterwards reſtored to its place with the former, which had been left in the out paſſage; and after re-adjuſtment they both agreed to 1°. This being obſerved for ſome days, one of them was taken down, in order to be packed up for London; this I have now the honour of exhibiting to the Royal Society; and I beg to leave in the Society's houſe, that in caſe any one ſhould be deſirous of having an inſtrument made on the ſame plan, they may have recourſe thereto.

It appears from the foregoing obſervations, that, in the compaſs of eleven months, the cords had ſtretched the value of 5°: and I alſo obſerved that they both had contracted their compaſs about 10°. I would therefore recommend, that an hygrometer ſhould, from its firſt adjuſtment, be re-adjuſted at the end of three months, and again at the end of ſix months from the firſt; after that, at the interval of about ſix months, to the end of two years from the beginning; and after that, I apprehend that once a year will ſuffice; the beſt time of adjuſtment being in the dry and warm weather of July or Auguſt: and by theſe means, I apprehend the inſtrument will be always kept within 2° of its proper point.

Reſpecting the ſenſibility of this inſtrument, it has that in a greater degree than its conſtancy to its ſcale can be depended upon, which was all that I intended; where greater degrees of ſenſibility are required, to make compariſons at ſmall intervals of time,

time, the beard of a wild oat, and other conſtructions, may be uſed with advantage; this inſtrument being conſidered as a check upon them as to more diſtant periods.

General Conclusion.

I am aware that an hygrometer, actuated by any principle of the kind here made uſe of, may not be a meaſurer of the quantity of moiſture, actually diſſolved in, and intimately mixed with the air; but only indicates the diſpoſition of the air to part with, or precipitate the water contained in its ſubſtance; or, on the contrary, to diſſolve and imbibe a greater quantity: but, as it is by ſeparating the effects of natural cauſes that we are enabled to judge of theſe cauſes, and from thence their effects when again compounded, every attempt to aſcertain the operations of a ſimple cauſe will have its value in the ſearch into nature: nor can we *a priori* determine the value of any new inſtrument; for, if it ſhould lead to a ſingle diſcovery, or even to aſcertain a ſingle fact, this may again lead to others of great importance, of which we might have either none, or an imperfect idea before. For my own part, I have always looked on a thick fog, and the ſweating or condenſation of the water's vapours upon the walls in the inſide of buildings, to be the greateſt marks of a moiſt atmoſphere: whereas I have not always found the hygrometer affected at theſe times in the higheſt degree. On the contrary, at the cloſe of a fine day, and the fall of the dew on the ſudden approach of a froſt, I have found the hygrometer more affected by moiſture than in ſome of the preceding caſes, and ſtill more by a falling dew in the time of an hard froſt. I juſt mention theſe matters as hints for the inquiry of others; not having had length of time, ſince I brought the inſtrument to anſwer my intention, to make any abſolute concluſions.

I am ſorry I have been obliged to take up ſo much compaſs, to deſcribe and explain a very ſimple inſtrument; but as I meant at the ſame time to give ſome idea of what is to be expected from it, I thought it more excuſeable to be prolix than not ſufficiently explicit.

J. SMEATON.

London, March 21, 1771.

P. S. It is to be noted, that, after each re-adjuſtment, though the hygrometers would generally within a few hours come near their point, yet it was not till the next day that they could be depended on, as having come to their neareſt agreement.

OBSERVATIONS on the Graduation of Astronomical Instruments; with an Explanation of the Method invented by the late Mr. HENRY HINDLEY, of York, Clock-maker, to divide Circles into any given Number of Parts. By Mr. JOHN SMEATON, F. R. S.; communicated by HENRY CAVENDISH, Esq. F. R. S. and S. A.

Read November 17, 1785.

PERHAPS no part of the ſcience of mechanics has been cultivated by the ingenious with more aſſiduity, or more deſervedly ſo, than the art of dividing circles for the purpoſes of aſtronomy and navigation. It is ſaid that TYCHO BRAHE and HEVELIUS laboured this part of their inſtruments with their own hands; and though public rewards have at length brought forth different methods of dividing from our beſt artiſts, which have been communicated to the public; yet I truſt it will be thought, that if any thing relative to this buſineſs remains yet behind, that may tend to furniſh the ingenious artiſts, who are cultivating this field, with any new or curious idea upon the ſubject, it will be well worth communicating to this learned Society: ſince, if an hint, which is eſſentially different from any thing that (ſo far as I know) the public is in poſſeſſion of, be once ſtarted, and is purſued and worked upon by ingenious men, it is not poſſible to ſay, to what valuable purpoſes it may be converted.

This, perhaps, will better appear by taking a ſhort review of the labours of others, from the time of TYCHO BRAHE and HEVELIUS (who did not uſe teleſcopic ſights) to the preſent time.

The very learned, ingenious, and inventive Dr. HOOK, in his Animadverſions on the *Machina Cœleſtis* of HEVELIUS, publiſhed in the year 1674, has given us an elaborate deſcription of a quadrant, whoſe diviſions were formed, and afterwards read off, by means of an endleſs ſcrew, working upon the outermoſt border of the limb of a quadrant; which, he ſays, *does not at all depend upon the care and diligence of the inſtrument-maker, in dividing, graving, or numbering the diviſions, for the*

the same screw makes it from end to end; yet he has given us no account of any particular care or caution that he uſed, in preventing the ſame ſcrew from making larger or ſmaller paces, in conſequence of unequal reſiſtance, from a different hardneſs of the metal in different parts of the limb; nor any method of correcting or checking the ſame; nor of making a ſcrew, the angle of whoſe threads with the axis ſhall be equal in every part of the circumference; therefore the whole of this buſineſs (in which accurate *mechanists* well know conſiſts the whole of the difficulty) he refers to the *ingenious workman;* and, in particular, to the then celebrated Mr. TOMPION, whom, he ſays, he employed to make his inſtrument, and who had thereby *ſeen and experienced the difficulties that do occur therein:* but were any ingenious workman now to purſue the directions of Dr. HOOK, ſo far as his communication thereof extends, we may conclude, that he would make a very inaccurate piece of work, far inferior in performance to what the Doctor ſeems to expect from it*. But yet, I believe, it was the *firſt* attempt to apply the endleſs ſcrew and wheel, or arch, to the purpoſe of forming diviſions for aſtronomical inſtruments; for, the Doctor ſays himſelf, the perfection of this inſtrument is the *way* of making the diviſions; that it *excels all the common ways of division:* and in the table of contents it is intituled, *An Explication of the new Way of dividing.*

This method, however, of Dr. HOOK's was not laid aſide without a very full and ſufficient trial: for Mr. FLAMSTEED, in the *Prolegomena* of the third volume of *Historia Cœlestis*, informs us, that *he* contrived the ſextant, wherewith his obſervations were chiefly made, from his entrance into the Royal Obſervatory in the year 1676 to the year 1689. This ſextant was firſt made of wood, and afterwards of iron, with a braſs limb of two inches broad, by Mr. TOMPION, at the expence of Sir JONAS MOORE; the radius thereof was 6 feet 9¼ inches; it was furniſhed with an endleſs ſcrew upon its limb of 17 threads in an inch, and with teleſcopic ſights†. Of this inſtrument Mr. FLAMSTEED gives the figure at the latter end of his *Prolegomena* before-mentioned, ſufficiently large to ſee the general deſign; the whole being mounted upon a ſtrong *polar axis* of iron, of three inches diameter.

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* This was indeed verified in an attempt upon the same plan by the DUC DE CHAULNES, published in a Memoir of the Royal Academy of Sciences at Paris, for the year 1765.

† "—Qualem nemo, cœlo adhibens;—" Preface to Historia Cœlest. printed in one vol. 1712.

Though,

Though, in the full deſcription of this inſtrument, Mr. FLAMSTEED mentioned the limb's being furniſhed with *diagonal diviſions*, diſtinguiſhing the arch to 10 ſeconds *; yet it is pretty clear, that it had not theſe originally upon it; but that the dependance was wholly upon the ſcrew diviſions, when it came out of Mr. TOMPION's hands. This one may reaſonably infer from the obſervations themſelves; for the firſt obſervation, ſet down as taken with this inſtrument, being upon the 29th of October, 1676, it was not till the 11th of September, 1677, that the column which contained the *check* angle by diagonal lines was filled up; and there was alſo a ſpace of time, antecedent to that laſt mentioned, wherein no obſervations are recorded as taken with this inſtrument, in which time the diagonal diviſions might be put on; and this will be put beyond a doubt, as he ſays expreſsly, that finding, in the year 1677, that the threads of the ſcrew had worn the border of the limb, he divided the limb into degrees himſelf, and drew a ſet of diagonal diviſions †; and then comparing the two ſets of diviſions together, he ſometimes found them to differ a whole minute; wherefore, for correction thereof, he conſtructed a new table for converſion of the revolutions and parts of the ſcrew into degrees, minutes, and ſeconds; and which he applied in the obſervations taken in 1678.—However, notwithſtanding this correction, in looking over the obſervations noted down as deduced each way, I frequently find a difference of half a minute; not unfrequently 40″; but in an obſervation of the moon, of the 9th June, 1687, I find a difference of 55″ ‡, which upon a radius of 6 feet 9 inches amounts to more than $\frac{1}{50}$th part of an inch.

In the year 1689, Mr. FLAMSTEED completed his *mural arc* at GREENWICH; and, in the *Prolegomena* before-mentioned, he makes an ample acknowledgement of the particular aſſiſtance, care, and induſtry of Mr. ABRAHAM SHARP; whom, in the month of Auguſt, 1688, he brought into the Obſervatory, as his *amanuensis*; and being, as Mr. FLAMSTEED tells us, not only a very ſkilful mathematician, but exceedingly expert in mechanical operations §, he was principally employed in the conſtruction of the mural arc; which in the compaſs of fourteen months he finiſhed, ſo

* Prolegomena Histor. Cœlest. vol. III. p. 104.

† Ibid. p. 106. "Gradus in limbo destribui ac diagonales duxi."

‡ Vol. I. of Hist. Cœlest. p. 343.

§ "Qui mechanices per quam expertus, pariter ac matheseos peritus." Prolegomena, vol. III. p. 108.

greatly

greatly to the ſatisfaction of Mr. FLAMSTEED, that he ſpeaks of him in the higheſt terms of praiſe *.

This celebrated inſtrument, of which he alſo gives the figure at the end of the *Prolegomena*, was of the radius of 6 feet 7½ inches; and in like manner as the ſextant was furniſhed both with ſcrew and diagonal diviſions, all performed by the accurate hand of Mr. SHARP. But yet, whoever compares the different parts of the table for converſions of the revolutions and parts of the ſcrew belonging to the mural arc into degrees, minutes, and ſeconds †, with each other, at the ſame diſtance from the zenith on different ſides; and with their halves, quarters, &c. will find as notable a diſagreement of the ſcrew-work from the hand-diviſions, as had appeared before in the work of Mr. TOMPION: and hence we may conclude, that the method of Dr. HOOK, being executed by two ſuch maſterly hands as TOMPION and SHARP, and found defective, is in reality not to be depended upon in nice matters.

From the account of Mr. FLAMSTEED it appears alſo, that Mr. SHARP obtained the zenith point of the inſtrument, or *line of collimation*, by obſervation of the zenith ſtars, with the face of the inſtrument on the eaſt and on the weſt ſide of the wall ‡: and that having made the index ſtronger (to prevent flexure) than that of the ſextant, and thereby heavier, he contrived, by means of pullies and balancing weights, to relieve the hand that was to move it from a great part of its gravity ‡.

I have been the more particular relating to Mr. SHARP, in the buſineſs of conſtructing this mural arc; not only becauſe we may ſuppoſe it the firſt good and valid inſtrument of the kind, but becauſe I look upon Mr. SHARP to have been the firſt perſon that cut accurate and delicate diviſions upon aſtronomical inſtruments; of which, independent of Mr. FLAMSTEED's teſtimony, there ſtill remain conſiderable proofs: for, after leaving Mr. FLAMSTEED, and quitting the department above-

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* "SHARPEIUS ſervus meus fidelissimus, ac omnibus quidem dotibus & facultatibus erat imbutus, "quæ ipsum tam subtili & difficili operi obeundo idoneum redderent." Prolegom. ibid.

And on finishing the instrument, he says, "Gradus describuntur sive numerantur et exsculpantur, "artificiosa manuali opera dicti domini SHARP, qui limbum partitus est, diagonales duxit, totumque "organum absolvit et perfecit: adeo ut præstantissimi quivis artifices postquam illud conspexerunt et "considerarunt, se exactius id peragere non potuisse, agnoverint." Prolegom. p. 111.

† Hist. Cœlest. vol. II. Appendix.

‡ Prolegom. p. 109.

mentioned,

mentioned *, he retired into Yorkſhire, to the village of Little Horton, near Bradford, where he ended his days about the year 1743; and where I have ſeen not only a large and very fine collection of mechanical tools (the principal ones being made with his own hands), but alſo a great variety of ſcales and inſtruments made therewith, both in wood and braſs, the diviſions whereof were ſo exquiſite, as would not diſcredit the firſt artiſts of the preſent times: and I believe there is now remaining a quadrant, of four or five feet radius, framed of wood, but the limb covered with a braſs plate; the ſubdiviſions being done by diagonals, the lines of which are as finely cut as thoſe upon the quadrants at Greenwich. The delicacy of Mr. SHARP's hand will indeed permanently appear from the copper-plates in a quarto book, publiſhed in the year 1718, intituled, *Geometry improved by A. Sharp, Philomath.* whereof not only the geometrical lines upon the plates, but the whole of the engraving of letters and figures, were done by himſelf, as I was told by a perſon in the mathematical line, who very frequently attended Mr. SHARP in the latter part of his life. I therefore look upon Mr. SHARP as the firſt perſon that brought the affair of hand diviſion to any degree of perfection.

Some time about the eſtabliſhment of the mural arc at Greenwich, the celebrated Daniſh Aſtronomer OLAUS ROEMER began his domeſtic Obſervatory, which he finiſhed in the year 1715, as we are informed by his hiſtorian PETER HORREBOW, in the third volume of his works, in the tract, intituled, *Basis Astronomiæ*, publiſhed in the year 1741. In this tract is the deſcription of an inſtrument, Table III. which not only anſwered the purpoſe of the meridian arc; but, its teleſcope being mounted upon a long axis, became alſo in reality what we now call a *Transit Inſtrument*; and which furniſhed, ſo far as I have been able to learn, the firſt idea thereof. One end of the axis of this inſtrument being the centre of the meridian arc, and carrying its index, M. ROEMER thereby avoided the errors ariſing from the plane of the mural arc not being accurately a vertical plane; and which Mr. FLAMSTEED endeavoured to check, by obſerving the paſſage of known ſtars nearly in the ſame parallel of declination; that is, paſſing nearly over the ſame part of the plane of the arc; by which he was enabled to correct or check the errors of the arc in right aſcenſion. But it is the peculiar method in which ROEMER *divided* his inſtruments, that occaſions him here to be introduced.

* Mr. SHARP continued in ſtrict correſpondence with Mr. FLAMSTEED ſo long as he lived, as appeared by letters of Mr. FLAMSTEED's found after Mr. SHARP's death; many of which I have ſeen.

Though

Though it is a very ſimple problem by which geometricians teach how to divide a given right line into any number of parts required; yet it is ſtill a much more ſimple thing to ſet off upon a given right line, from a point given, any number of equal parts required, where the total length is not exactly limited; for this amounts to nothing more than aſſuming a convenient opening of the compaſſes, and beginning at the point given, to ſet off the opening of the compaſſes as many times in ſucceſſion, as there are equal parts required; which proceſs is as applicable to the arch of a circle as it is to a right line. Of this ſimple principle ROEMER endeavoured to avail himſelf.

For this purpoſe M. ROEMER took two ſtiff, but very fine pointed, pieces of ſteel, and fixed them together, ſo as to avoid, as much as poſſible, every degree of ſpring that would neceſſarily attend long-legged compaſſes, or even thoſe of the ſhorteſt and ſtiffeſt kind when the points are brought near together. The diſtance of the points that he choſe was about the $\frac{1}{170}$ or $\frac{1}{172}$ of an inch. This, upon a radius of $2\frac{1}{2}$ or three feet, would be about 10 minutes. With this opening, beginning at the point given, he ſet off equal ſpaces in ſucceſſion to the end of his arch, which was about 75°. Thoſe were diſtinguiſhed upon the limb of the inſtrument by very fine points, which were referred to by a groſſer diviſion, the whole being properly numbered. The ſubdiviſion of thoſe arches of 10 minutes each was performed by a double microſcope, carried upon the arm or radius of the inſtrument, the common focus being furniſhed with parallel threads of ſingle ſilk, whereof eleven being diſpoſed at ten equal intervals, comprehending together one ten-minute diviſion, the diſtance of the neareſt threads became a very viſible ſpace, anſwerable to one minute each, and therefore capable of a much further ſubdiviſion by eſtimation.

The diviſions of this inſtrument were therefore, properly ſpeaking, not degrees and minutes; but yet, if exactly equal, would ſerve the purpoſe as well, when their true value was found, which was done by compariſon with larger inſtruments.

Now, if it be conſidered, that in going ſtep by ſtep of ten minutes each, through a ſpace of 75 degrees, there will be a ſucceſſion of 450 diviſions, dependant upon each other; if it be alſo conſidered, that the leaſt degree of extuberance in the ſurface of the metal, where each new point is ſet down, or the leaſt hard particle (wherewith all the baſe metals ſeem to abound) will cauſe a deviation in the firſt impreſſion of a taper point, and thereby produce an inequality in the diviſion; it is evident, that though this inequality may be very ſmall, and even imperceptible between neighbouring

ing diviſions, yet among diſtant ones, it may and will ariſe to ſomething conſiderable; which, in the menſuration of angles, will have the ſame ill tendency as in near ones. Now, as M. ROEMER has given us no means of checking the diſtant diviſions, in reſpect of each other, it is very probable that no one has followed his ſteps, in caſes where great accuracy was required, in a conſiderable number of diviſions. For in reality this method is likely to fall far ſhort of Dr. HOOK's; as Dr. HOOK's diviſions being cut in a ſimilar ſucceſſive manner, by the rotation of the ſharp *edge* of the threads of a ſcrew againſt the exterior edge of the limb of the inſtrument, a very ſlight degree of preſſure will bring a fine ſcrew of thirty threads in an inch (which he preſcribes) to touch againſt an arch whoſe radius is four or five feet in more than one, two, three, or four threads at once; ſo that the threads ſupporting one another, a ſmall extuberance, or even a ſmall hard particle in the metal, will be cut through or removed by the grinding or rather ſawing motion of the ſcrew; and which, in regard to its contact, being in reality an edge, will be much more effectual (that is, more firm) in its retention than a mere ſimple point: and a repetition of the operation, from the ſupport of the threads to each other, will tend to mend the firſt traces; whereas, in ROEMER's way, a repetition will make them worſe; for, whatever drove forward or backward the point on firſt entering, will, from the ſloping of the point, be confirmed and increaſed in driving it deeper.

When Dr. HALLEY was choſen Aſtronomer Royal (Mr. FLAMSTEED's inſtruments being taken away by his executors), Mr. GRAHAM undertook to make a new mural quadrant, about the year 1725; who, uniting all that appeared valuable in the different methods of his predeceſſors, executed it with a degree of contrivance, accuracy, and preciſion, before unknown: and the diviſion thereof he performed with his own hand. The model of this quadrant, for ſtrength, eaſy management, and convenience, has been ever ſince purſued as the moſt perfect. What I apprehend to be peculiar in it, was the application of the arch of 96°; not only as a check upon the arc of degrees and minutes, but as ſuperior thereto, being derived from the more ſimple principle of *continual biſection.*

To make room for this, he has entirely rejected the ſubdiviſion by diagonals, and has adopted the method of the *Vernier;* but the ſubdiviſion of the vernier diviſions he, as I apprehend for the firſt time, meaſured by the turns of the detached adjuſting ſcrew, making it in fact a micrometer, by which the diſtance of the *ſet* of the inſtrument was to be meaſured from the perfect coincidence of one of the actual diviſions of the limb with the next ſtroke of the vernier; by which means the obſervation

tion could not only be read off with all the preciſion that the diviſion of the inſtrument was capable of, but the two ſets of diviſions could be checked and compared with each other. Another thing that I apprehend to be peculiar in this inſtrument, was the more certain method of transferring and cutting the diviſions, from the original divided points, by means of the *beam-compass*, than could poſſibly be done from a *fiducial edge*, as had doubtleſs been conſtantly the practice in cutting diagonals; for, placing the ſteady point of the beam-compaſs in the tangent line to that part of the arc where each diviſion was to be cut, the opening of the compaſs being nearly the length of the tangent, the other point would cut the diviſion in the direction of the radius nearly; and though in reality an arch of a circle, yet the ſmall part of it in uſe would be ſo nearly a right line, as perfectly to anſwer the ſame end; all which advantages put together, it is probable induced Mr. GRAHAM to reject the diagonals.

Soon after the completion of this quadrant, Mr. GRAHAM undertook to execute a *zenith ſector* for the Rev. Dr. BRADLEY, which was fixed up at Wanſtead, in Eſſex, in the year 1727. The very ſimple conſtruction that he adopted for this inſtrument (the plumb line itſelf being the index) did not admit of the uſe of a vernier: he therefore contented himſelf with dividing the arch of the limb of this inſtrument by primary points, as cloſe as he thought neceſſary, that is, by diviſions of five minutes each, and meaſuring the diſtance from the *ſet* of the inſtrument to the next point of diviſion by a *micrometer* ſcrew, in the conſtruction of which ſcrew he uſed uncommon care and delicacy. I have mentioned this inſtrument to introduce this obſervation; that I think it highly probable, had Mr. GRAHAM conſtructed the great quadrant *after* the zenith ſector had been fully tried, he would have rejected not only the diagonals but the verniers alſo, as containing a ſource of error within themſelves which may be avoided by a well-made ſcrew *.

It ſeems alſo, that Mr. GRAHAM, at the time he conſtructed both theſe inſtruments, was not aware how much error could ariſe from the unequal expanſions of different metals by heat or cold: for in both the radius, or frame of the inſtrument, was iron,

* This has been found conſentaneous to the experience of my friend Mr. AUBERT, who, on my ſuggeſtion, has long ſince laid aside the uſe of his vernier, meaſuring always by the micrometer ſcrews the diſtance between the ſet of the inſtrument, and the coincidence of the firſt ſtroke of the vernier with the next primary diviſion of the limb.

while the limbs were of braſs. They, however, remain in the Royal Obſervatory, perfect models, in all other reſpects, of every thing that is likely to be attained in their reſpective deſtinations, and monuments of the ſuperlative abilities of that great mechanician Mr. GRAHAM *.

Mr. GRAHAM lived till the year 1751; and during his time there were few inſtruments of conſequence conſtructed without his advice and opinion. They were for many years done by Mr. SISSON, to whom doubtleſs Mr. GRAHAM would fully communicate his method of diviſion; and from this ſchool aroſe that very eminent and accurate artiſt Mr. BIRD, whoſe delicate hand, joined with great care and aſſiduity, enabled him ſtill further to promote this branch of diviſion; and which, being carried by him to a great pitch of perfection, the Commiſſioners of Longitude did themſelves the credit, by an handſome reward, to induce him to publiſh to the world his particular method of dividing aſtronomical inſtruments; which being drawn up by himſelf, in the year 1767, this matter is fully ſet forth to the public: I ſhall therefore only take this opportunity of obſerving, that there ſeems to be one article in which Mr. BIRD's method may be ſtill improved.

I muſt here obſerve, that I apprehend no quadrant, that has ever undergone a ſevere examination, has been found to form a perfect arch of 90°; nor is it at all neceſſary it ſhould: the perfect equality of the diviſions throughout the whole is the firſt and primary conſideration; as the proportion of error, when aſcertained by proper obſervations, can be as eaſily and readily applied, when the whole error of the rectangle is fifteen ſeconds, as when it is but five.

In this view, from the radius taken, I would compute the chord of ſixteen degrees only. If I had an excellent plain ſcale, I would uſe it; becauſe I ſhould expect the deviation from the right angle to be leſs than if taken from a ſcale of more moderate accuracy; but if not, the equality of the diviſions would not be affected, though taken from any common diagonal ſcale. This chord, ſo prepared, I would lay off five times in ſucceſſion, from the primary point of o given, which would complete eighty degrees; I would then biſect each of thoſe arches of 16°, as preſcribed by

* I have been informed, that Dr. MASKELYNE has caused this objection to the sector to be rectified, since its removal to the Royal Observatory, by substituting an iron limb instead of that of brass, the points being made upon studs of gold.

Mr.

Mr. BIRD, and laying off one of them beyond the 80th, would give the 88th degree: proceeding then by biſection, till I came to an arch of two degrees, laying that off from the 88th degree, would give the point of ninety degrees. Proceeding ſtill by biſection, till I had reduced the degrees into quarters = fifteen minutes each, I would there ſtop; as from experience I know, that when diviſions are over cloſe, the accuracy of them, even by biſection, cannot be ſo well attained as where they are moderately large. If a ſpace of $\frac{4}{10}$ of an inch, which is a quarter of a degree, upon an eight feet radius, is thought too large an interval to draw the index over by the micrometer ſcrew, this may be ſhortened by placing another line at the diſtance of one-third of a diviſion on each ſide of the index line, in which caſe the ſcrew will never have to move the index plate more than one-third of a diviſion, or five minutes; and the perfect equality of thoſe ſide lines from the index line may be obtained, and adjuſted to five minutes preciſely, by putting each of the ſide lines upon a little plate, capable of adjuſtment to its true diſtance from the middle one, by an adjuſting ſcrew.

The above hint is not confined to the chord of ſixteen degrees, which prohibits the ſubdiviſions going lower than fifteen minutes: for if it be required to have diviſions equivalent to five minutes upon the limb itſelf, then I would compute the chord of 21° 20′ *only*; and laying it off four times from the primary point, the laſt would mark out the diviſion 85° 20′, pointed out by Mr. BIRD; *ſupplying the remainder* to a quadrant, from the biſected diviſions as they ariſe, and not by the application of other *computed* chords.

In my Introduction to M. ROEMER's Method of Diviſion, I have ſhewn, that diviſions laid off in ſucceſſion, by the ſame opening of the compaſſes, either in a right line, or in the arch of a circle, being in its idea geometrically true, and in itſelf the moſt ſimple of all proceſſes, it has the faireſt chance of being mechanically and practically exact, when cleared of the diſturbing cauſes. The objection therefore to his method is, the great number of repetitions, which depending upon each other in ſucceſſion (requiring no leſs than 540 to a quadrant, when ſubdivided to ten minutes each), the ſmalleſt error in each, repeated 540 times, without any thing to check it by the way, may ariſe to a very ſenſible and large amount: but in the method I have hinted, this objection will not lie; for, in the firſt caſe, the aſſumed opening is laid off but five times; and in the latter caſe but four times; nor does this *repetition* ariſe out of the nature of the thing; for, if you like it better, you may, in the former caſe, at once compute the chord of 64°; and in the latter that of 85° 20′, and then proceed

proceed wholly by biſection; ſupplying what is wanted to make up the quadrant, from the biſected diviſions, as they ariſe. Mr. BIRD preſcribes this method himſelf, for the diviſion of HADLEY's ſectants and octants.

He, I ſuppoſe, was the firſt, who conceived the idea of laying off chords of arches, whoſe ſub-diviſions ſhould be come at by continual biſection; but why he mixed therewith diviſions that were derived from a different origin (as preſcribed in his method of dividing) I do not eaſily conceive. He ſays, that after he had proceeded by the biſections, from the arc of 85° 20′, the ſeveral points of 30°, 60°, 75°, and 90°, (all of which were laid down from the principle of the chord of 60 being equal to radius), *fell in without ſensible inequality*; and ſo indeed they might; but yet it does not follow that they were equally true in their places as if they had been (like the reſt) laid down from the biſection from 85° 20′, and therefore being the firſt made, whatever error was in them, would be communicated to all connected with them, or taking their departure from them. Every heterogeneous mixture ſhould be avoided where equal diviſions are required. It is not the ſame thing (as every good artiſt will ſee) whether you *twice* take a meaſure from a ſcale as *nearly* the ſame as you can, and lay them off ſeparately; or lay off *two* openings of the compaſſes, in ſucceſſion, *unaltered*; for though the ſame opening, carefully taken off from the ſame ſcale a ſecond time, will doubtleſs fall into the points made by the firſt, without ſenſible error; yet as the ſloping ſides of the conical cavities made by the firſt point will conduct the points themſelves to the centre, there may be an error which, though inſenſible to the ſight, would have been avoided by the *more simple process* of laying off the opening twice, without ever altering the compaſſes.

The 96 arc was, I have no doubt, invented by Mr. GRAHAM, from having perceived, in common with all preceding artiſts, how very much more eaſy a given line was to biſect, than to triſect, or quinqueſect; and therefore the 96 arc which proceeded by biſections only (or by laying off the ſame identical openings, which, as already ſhewn, is ſtill more ſimple and unexceptionable) was wholly intended by him by way of checking the diviſion of the arc of 90, which required triſections and quinqueſections. But experience ſoon ſhewed the ſuperior advantage of it ſo ſtrongly, that the uſe of the 90 arc is now wholly ſet aſide, where accuracy is required; whereas the ingenuity of Mr. BIRD having ſhewn a way to produce the 90 arc by biſection, when this is really purſued quite through the piece, by rejecting all diviſions derived from any other origin, the 90 arc will have nothing in it to prevent its being equally unexceptionable

tionable with the 96 arc; and conſequently if, inſtead of the 96 arc, another arc of 90 was laid down (which being upon a different radius, its diviſions will ſtand totally unconnected with the former), then theſe two arcs would in *reality* be a check upon each other; for being of equal validity, the mean might be taken: and if (in lieu of vernier diviſions) ſtrokes at the diſtance of any odd number, as 7, 9, 11, or 13, are marked upon, and carried along with, the index plate; theſe will produce a check upon neighbouring diviſions; and the angle may then be deduced from the medium of no leſs than four readings.

The laſt works that have been made known to the public in the line of graduation (ſo far as has come to my knowledge) are thoſe of the very ingenious Mr. RAMSDEN, which were publiſhed, by order of the Board of Longitude, in the year 1777.

From his own information, I learn, that in the year 1760, he turned his thoughts towards making an engine for dividing mathematical inſtruments; and this he did in conſequence of a reward offered by the Board of Longitude to Mr. BIRD, for publiſhing his method of graduating quadrants; for as, ſeveral years previous to that period, he had taken great pains to accompliſh himſelf in the art of hand-dividing, in which line Mr. BIRD had acquired his eminence, he conceived by this publication of Mr. BIRD's he ſhould be reduced to the ſame ſtandard of performance with the reſt of the trade. He, therefore, partly to ſave time, and that kind of wearineſs to an ingenious mind that ever muſt attend the endleſs repetition of the ſame thing from morning to night; partly ſtill to preſerve the pre-eminence he had then gained; and partly to procure diſpatch in the great increaſe of demand for HADLEY's ſextants and octants, in conſequence of the ſucceſsful application of the moon's motion to the purpoſe of aſcertaining the longitude at ſea (which inſtruments for this purpoſe required a degree of accuracy and certainty in the diviſion, by no means neceſſary thereto when applied to the ſimple purpoſe of obſerving latitudes); I ſay, for theſe conſiderations, Mr. RAMSDEN determined to ſet about ſomething in the inſtrumental way, that ſhould be ſufficient effectually to anſwer theſe purpoſes.

Accordingly, conſidering the nature of the endleſs ſcrew, he ſet himſelf to work upon an engine whoſe divided wheel or plate was of thirty inches diameter; and though the performance of this firſt eſſay was inferior to his expectations and wiſhes, yet, with it, he was able to divide theodolites with a degree of preciſion far ſuperior to any thing of the kind that had been exhibited to the public.

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This engine I myſelf ſaw in the ſpring of the year 1768; and it appeared to me not only a very laudable attempt towards inſtrumental diviſions, but a very good model for the conſtruction of an engine of the moſt accurate kind for that purpoſe. And he furthermore, at the ſame time, ſhewed me the model or pattern for caſting a wheel of a much larger ſize, which he propoſed to make upon the ſame plan, and with conſiderable improvements. This being effected ſome time in or about the year 1774, its accuracy was proved by making a ſextant, afterwards ſubjected to the examination of Mr. BIRD; who in conſequence approved the method, not only as fully ſufficient for the diviſion of HADLEY's ſextants and octants for any purpoſe whatſoever, but in fact for dividing any inſtrument whoſe radius did not exceed that of the dividing wheel, which was forty-five inches in diameter: whereupon the Board of Longitude, ever ready to encourage all endeavours that tend to the certainty and perfection of any thing ſubſervient to the purpoſe of finding the longitude at ſea, very properly and uſefully reſolved to confer an handſome reward on Mr. RAMSDEN, for delivering a full explanation of his method of making the ſaid engine; which, in conſequence, was publiſhed by order of the Board of Longitude in the year already mentioned, viz. 1777: the deſigns whereof are ſo full and explicit, that whoever could not underſtand that deſcription, ſo as to enable him to make it, would be unfit to undertake it on other accounts.

From what I have ſaid upon the works of the different artiſts that I have mentioned, it would ſeem that the art of graduation was brought to that degree of perfection, that nothing material can now be added thereto: and I ſhould have been apt to have thought ſo myſelf, if I had not happened, in the courſe of my life, to have had a communication made to me (under the ſeal of ſecrecy) which ſeems to promiſe yet further light and aſſiſtance in perfecting that important art; and every impediment to the diſcovery thereof being now removed, I ſhall in the remainder of this eſſay give the cleareſt deſcription thereof that I am able, with ſuch elucidations and improvements as ſeem to be naturally pointed out by the method itſelf.

In the autumn of the year 1741, I was firſt introduced to the acquaintance of that then eminent artiſt, Mr. HENRY HINDLEY, of York, clock-maker;—he immediately entered with me into the greateſt freedom of communication, which founded a friendſhip that laſted to his death, which did not happen till the year 1771, at the age of 70. On the firſt interview, he ſhewed me not only his general ſet of tools, but his *engine*, at that time furniſhed with a dividing plate, with a great variety of numbers for cutting the teeth of clock wheels, &c. and alſo, for more nice and curious purpoſes,

poſes, furniſhed with a wheel of about thirteen inches diameter, very ſtout and ſtrong, and cut into 360 teeth; to which was applied an endleſs ſcrew, adapted thereto. The threads of this ſcrew were not formed upon a cylindric ſurface, but upon a ſolid whoſe ſides were terminated by arches of circles. The whole length contained fifteen threads; and as every thread (on the ſide next the wheel) pointed towards the centre thereof, the whole fifteen were in contact together; and had been ſo ground with the wheel, that, to my great aſtoniſhment, I found the ſcrew would turn round with the utmoſt freedom, interlocked with the teeth of the wheel, and would draw the wheel round without any ſhake or ſticking, or the leaſt ſenſation of inequality.

How long this engine might have been made before this firſt interview, I cannot now exactly aſcertain: I believe not more than about a couple of years; but this I well remember, that he then ſhewed me an inſtrument intended for aſtronomical purpoſes, which muſt have been produced from the engine, and which of itſelf muſt have taken ſome time in making *.

I in reality thought myſelf much indebted to Mr. HINDLEY, for this communication; but though he ſhewed me his engine, and told me, that the ſcrew was cut by

* This inſtrument was of the equatorial kind; the wheel parallel to the equator, the quadrant of latitude, and ſemi-circle of declination, being all furniſhed with ſcrews containing fifteen threads each, framed and moved in the ſame manner as that of the engine; the whole of which inſtrument was already framed, and the telescope tube in its place, which was intended to be of the inverting refracting kind, and to be furniſhed with a micrometer. This, however, was not completed till ſome years after; but, in the year 1748, I received it in London for ſale. It ſtaid with me two years, in which time I ſhewed it to all my mechanical and philoſophical friends, amongſt whom was Mr. SHORT, who afterwards publiſhed in the Philoſophical Tranſactions, vol. XLVI. No. 493, p. 241, an account of a portable Obſervatory, but without claiming any particular merit from the contrivance. However, the model of it differs from HINDLEY's equatorial only in the following articles. He added an azimuth circle and compaſs at the bottom. He omitted the endleſs ſcrews, placing verniers in their ſtead; and at the top a reflecting telescope inſtead of a refractor. This inſtrument of HINDLEY's being afterwards returned to him unſold, I pointed out the principal deficiencies that I found therein; viz. that, in putting the inſtrument into different poſitions, the ſpringing of the materials was ſuch as in ſome poſitions to amount to conſiderable errors. This remained with him in the ſame ſtate till the year of the firſt *Tranſit of Venus*, viz. 1761; when it was ſold to ——— CONSTABLE, Eſq. of *Burton Conſtable*, in *Holderneſs*. Mr. HINDLEY, to remedy the evil above-mentioned, applied balances to the different movements. He ſoon afterwards completed one, *de novo*, upon this improved plan, for his Grace the late Duke of NORFOLK. A method of balancing in much the ſame way, without the knowledge that it had been done before, has been fully explained, and laid before the Society, by our ingenious and worthy brother Mr. NAIRNE. Phil. Trans. vol. LXI. p. 108.

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the rotation of the point of a tool, carried round upon a ſtrong arm, at the diſtance of the radius of the wheel from the centre of motion, which arm was carried forward by the wheel itſelf, and the wheel was put forward by an endleſs ſcrew, formed upon a cylinder to a proper ſize of thread, cut by his chock lathe; though he ſhewed me alſo this chock lathe, and the method employed to make the threads of the ſcrew *equiangular* with the axis, that is, to free the ſcrew from what workmen term *drunkenneſs*; and alſo ſhewed me how, by the ſingle ſcrew of his lathe, he could cut, by means of wheel-work, ſcrews of every neceſſary degree of fineneſs * (and, by taking out a wheel, could cut a left-handed ſcrew of the very ſame degree of fineneſs); by which means he was enabled not only to adapt his plain ſcrew to the ſize of the teeth of his wheel, but alſo to prevent any drunkenneſs that otherwiſe the curved ſcrew would be ſubject to in conſequence of being produced from the plain one; furthermore, that the ſcrew and wheel, being ground together as an optic glaſs to its tool, produced that degree of ſmoothneſs in its motion that I obſerved; and, laſtly, that the wheel was cut from the dividing plate: yet, how the dividing plate was produced, he for particular reaſons reſerved to himſelf.

Nor can he be blamed for the reſervation of this one ſecret; as he had, even at the time of my early acquaintance with him, a kind of foreſight that from the ſuperior merit of HADLEY's quadrant, a demand for that, and other inſtruments for the purpoſe of navigation, was likely to increaſe; and that he might live to ſee a public reward offered for a method of dividing them with greater accuracy and diſpatch than had at that time appeared. Indeed, he had himſelf an idea, from the ſatisfactory ſucceſs that had attended his operations in dividing, that a ſcrew and wheel, produced from his engines of one foot diameter, would have as much truth as the eight-feet quadrant at Greenwich: and though he doubtleſs greatly over-rated the accuracy of theſe miniature performances, yet it does not follow, as his methods were not confined to ſo narrow a compaſs, but that, his ſcale of operation being proportionably enlarged, a degree of accuracy in the graduation of aſtronomical inſtruments may be attained in proportion.

I muſt here beg leave to obſerve, that there appears to me to be a natural limitation to the accuracy of inſtruments, conſiſting of conſiderable portions of a circle, ſuch

* A machine for cutting the endless screw of Mr. RAMSDEN's engine, upon principles exactly similar, is fully and accurately set forth in his Description of his dividing Engine above-mentioned.

ſuch as quadrants, &c.* I do not find that the fineſt ſtroke upon the limb of a quadrant, made by BIRD's own hand, if removed from its coincidence with its index, can be replaced with any degree of certainty nearer than the 4000dth part of an inch, though aided by a magnifying glaſs †.

A 4000dth part of an inch being then determined to be the *minium visibile* by the ſtrokes of an inſtrument, this will be leſs than one ſecond of a degree upon a radius of four feet; and therefore, if the whole ſet of diviſions upon the limb could be preſerved true to this aliquot part of an inch, the eight-feet quadrants of Greenwich might be expected to be true to half a ſecond. How far they are from this, I do not exactly know; but I have reaſon to think they vary from it ſome ſeconds: nay, I believe it is generally allowed, that our largeſt quadrants, even when executed by the accurate hand of Mr. BIRD, do not exceed thoſe of a leſs ſize, by the ſame hand, in proportion to their increaſe of radius: nor can it well be expected that they ſhould; ſince, as the weight neceſſarily increaſes in a triplicate ratio of the radius, the great weight of the Greenwich quadrants in moving and fixing them (as they could not be divided in their place) may eaſily derange the framing; or even the *internal elasticity* of the materials may give way, by a change of poſition, to ſo minute a quantity as a 4000dth part of an inch. It therefore appears to me, that ſince the diviſions of a quadrant of four feet radius are more than ſufficient, and even thoſe of three feet admit of all the diſtinctneſs that in other reſpects is wanted, a three feet quadrant, in point of ſize, is capable of all attainable exactneſs; and would be as much to be depended on as any of thoſe now in being of eight feet. By adopting quadrants of this ſmaller ſize, we ſhall of courſe get rid of $\frac{1}{19}$ths of the preſent weight; and conſequently of much cumber, unhandineſs, and derangement, that muſt ariſe from that weight, as well as the fear of totally diſcompoſing them, if ever moved out of their place.

It now comes to be time to open a principle upon which there is a proſpect of effecting ſuch an improvement. I have ſhewn that a 4000dth part of an inch is the

* The zenith ſector conſiſts but of *few* degrees, with little variation of its poſition in uſing it.

† It will be to little purpoſe to attempt it with a greater power. Double microſcopes can doubtleſs be formed to magnify objects, far leſs than a 4000dth part of an inch, to diſtinct ſurfaces; but then the advantage of ſuch degrees of magnifying power is chiefly upon the organized bodies of nature. Let a dot, or the fineſt point that can be made by human art, be ſo viewed, and it will appear not round, but a very ragged irregular figure.

ultimatum that we are to expect from *sight*, though aided by glaſſes, when obſerving the diviſions of an inſtrument. But in the XLVIIIth volume of the Philoſophical Tranſactions (page 149 of this volume) I have ſhewn the mechaniſm of a new *pyrometer*, and experiments made therewith; whereby it appears, that, upon the principle of *contact*, a 24,000dth part of an inch is a very definite quantity. I remembered very well that I did not then go to the extent of what I might have aſſerted, being willing to keep within the bounds of *credibility*: but on occaſion of the preſent ſubject, I have re-examined this inſtrument, and find myſelf very well authoriſed to ſay, that a 60,000th part of an inch, with ſuch an inſtrument, is a more definite and certain quantity than a 4000th part of an inch is to the *sight*, conditioned as above ſpecified. The certainty of contact is, therefore, fifteen times greater than that of viſion, when applied to the diviſions of an inſtrument: and if this principle of certainty in contact did not take place even much beyond the limit I have now aſſigned, we never ſhould have ſeen thoſe exquiſite mirrors for reflecting teleſcopes, that have already been produced.

Theſe reflections apply immediately to my preſent ſubject, as HINDLEY's method of diviſion proceeds *wholly by contact*, and that of the firmeſt kind; there being ſcarcely need of magnifying glaſſes in any part of the operation.

In the year 1748, I came to ſettle in London; and the firſt employment I met with was that of making philoſophical inſtruments and apparatus. In this ſituation, my friend HINDLEY, from a principle the reverſe of jealouſy, fully communicated to me, by letter, his method of diviſion; and though I was enjoined ſecrecy reſpecting others (for the reaſons already mentioned), yet the communication was expreſsly made with an intention that I might apply it to my own purpoſes.

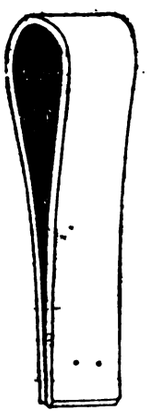
The following are extracts from two letters, which contain the whole of what related to this ſubject; and ſince I have many things to obſerve thereon, ſo that the paraphraſe would be much greater than the text, I think it beſt not to interrupt the deſcription with any commentary, as perhaps his own mode of expreſſion will more briefly and happily convey the general idea of the work than any I can uſe inſtead of it.

MY DEAR FRIEND, York, 14th Nov. 1748.

AS to what you was mentioning about my brother's knowing how I divided my engine plate, I will deſcribe it as well as I can myſelf; but you will want a good many things to go through with it.

The

The manner is this: firſt chooſe the largeſt number you want, and then chooſe a long plate of thin braſs; mine was about one inch in breadth, and eight feet in length, which I bent like an hoop for an hogſhead, and ſoldered the ends together; and turned it of equal thickneſs, upon a block of ſmooth-grained wood, upon my great lathe in the air (that is, upon the end of the mandrel): one ſide of the hoop muſt be rather wider than the other, that it may fit the better to the block, which will be a ſhort piece of a cone of a large diameter: when the hoop was turned, I took it off, cut, and opened it ſtraight again.



The next ſtep was to have a piece of ſteel bended into the form as *per margin* *; which had two ſmall holes bored in it, of equal bigneſs, one to receive a ſmall pin, and the other a drill of equal ſize. I ground the holes after they were hardened, to make them round and ſmooth. The chaps formed by this ſteel plate were as near together as juſt to let the long plate through. Being open at one end, the chaps ſo formed would ſpring a little, and would preſs the long plate cloſe, by ſetting in the *vise*. Then I put the long plate to a right angle to the length of the ſteel chaps, and bored one hole through the long plate, into which I put the ſmall pin; then bored through the other hole; and by moving the ſteel chaps a hole forward, and putting in the pin in the laſt hole, I proceeded till I had divided the whole length of the plate.

The next thing was to make this into a circle again. After the plate was cut off at the end of the intended number, I then proceeded to join the ends, which I did thus: I bored two narrow ſhort braſs plates † as I did the long one, and put one on the inſide, and the other on the outſide of the hoop, whoſe ends were brought together; and put two or three turned ſcrew pins, with flat heads and nuts to them, into each end, which held them together till I rivetted two little plates, one on each ſide of the narrow plate, on the outſide of the hoop. Then I took out the ſcrews, and turned my block down, till the hoop would fit cloſe on; and by that means my right line was made into an equal divided circle of what number I pleaſed.

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* The figure is conſiderably leſs than the real tool ſhould be.

† Theſe I ſhall hereafter diſtinguiſh by the name of *ſaddle plates*.

The

The engine plate was fixed on the face of the block, with a ſteel hole fixed before it, to bore through; and I had a point that would fall into the holes of the divided hoop; ſo by cutting ſhorter, and turning the block leſs, I got all the numbers on my plate.

I need not tell you, that you get as many prime numbers as you pleaſe; nor that the diſtance of the holes in the ſteel chaps muſt be proportioned to the length of the hoop.

You may aſk my brother what he knows about my method of dividing; but need not tell him what I have ſaid about it; for I think neither *he* nor *John Smith* know ſo much as I have *told you*, though I believe they got ſome knowledge of it in general terms *.——I deſire you to keep the method of dividing to *yourself*, and conclude with my beſt wiſhes,

And am, dear Sir, yours, &c.

HEN. HINDLEY.

Though the above letter was in itſelf very clear and explicit as to the general traces of the method, yet ſome doubts occurring to me, a further explanation became neceſſary. A copy of my letter not being preſerved, the purport of it may be inferred from the anſwer, which was as follows:

DEAR FRIEND, York, 13th March, 1748-9.

I THINK in your laſt you ſeem to be apprehenſive of ſome difficulties in drilling the hoop for dividing: Firſt, that the centre of the hole in the hoop might not be preciſely in the centre of the hole of the ſteel chaps, it was drilled in; but if I deſcribed fully to you the method I uſed, I can ſee no danger of error there: for my chaps were very thick, and the two correſponding holes were a little conical, and ground with a ſteel pin; firſt one pair, and then the other, alternately, till the pin would go the ſame depth into each. Then for drilling the hoop, I took any common drill that would paſs through, and bore the hole. After that I took a five-ſided

* The perſons here referred to were both bred with him. His brother, Mr. ROGER HINDLEY, who has many years followed the ingenious profeſſion of a watch-cap maker in London, was ſo much younger as to be an apprentice to him. Mr. JOHN SMITH, now dead, had ſome years paſt, the honour to work in the inſtrument way, under the direction of the late Dr. DEMAINBRAY, for his preſent MAJESTY.

broach,

broach, which opened the hole in the braſs betwixt the ſteel chaps, but would not touch the ſteel; ſo conſequently the centre of the holes in the braſs muſt be concentric with the holes in the chaps; and for alterations by air, heat, cold, &c. I was not above two or three hours in drilling a row of holes, as far as I remember.

2dly, For drilling, in a right line, I had a thin braſs plate, faſtened between the ſteel chaps, for the edge of the hoop to bear againſt, whilſt I thruſt it forward from hole to hole. What you propoſe of an iron frame with a lead outſide, will be better than my wooden block; but conſidering the little time that paſt, betwixt transferring the diviſions of the hoop to the diviſions of my dividing plate, I did not ſuffer much that way. It was when I drilled the holes in my dividing plate that I uſed a frame for drilling, which had one part of it that had a ſteel hole, that in lying upon the plane of the dividing plate was fixed faſt in its place for the point of the drill to paſs through: then, at the length of the drill, there was another piece of ſteel, with a hole in it, to receive the other end of the drill to keep it at right-angles to the plane of the plate. This piece was a ſpring, which bended at the end, where it was faſtened to the frame of the lathe, at about eighteen inches from the end of the drill; ſo it puſhed the drill through with any given force the drill would bear: and though that end of the drill moved in the arch of a circle, it was a very ſmall part of it, being no more than equal to the thickneſs of the dividing plate.

My good wiſhes conclude me yours,

HEN. HINDLEY.

Whoever attentively conſiders the communication contained in the above letters will ſee, that more happy expedients could not have been deviſed to procure a ſet of diviſions, where there ſhould be the moſt exact equality among *neighbours*; and which, for the purpoſes of clock-making, is the principal thing to be wiſhed for. But herein, as in M. ROEMER's method, there were no means of checking the diſtant diviſions, which run on to 360: now ſuch a check, when the expanſion of metals is conſidered, and particularly the difference of expanſion between braſs and ſteel, ſeems abſolutely neceſſary for the purpoſe of diviſions upon inſtruments, where the accurate menſuration of large angles is required, as much as the equality of neighbouring diviſions *.

* The ingenious Mr. STANCLIFFE (some years a workman of HINDLEY's) has suggested, that the difference of expansion between the steel chaps and the brass hoop may be avoided by making the chaps of brass also, with hard steel holes set separately therein, somewhat similar to the jewelled holes of watches.

With

With this view the invention of this ingenious perſon ſuggeſted to him the thought of making his curved ſcrew to lay hold of fifteen teeth or degrees together: this, in effect, becomes a pair of compaſſes, 24 removes of which complete the whole circle, and produce 24 checks in the circumference: and whoever conſiders the very exquiſite degree of truth that reſults from the grinding of ſurfaces in contact, as already noticed, muſt expect a very great degree of rectification of whatever errors might ſubſiſt in the wheel after its firſt cutting.

What degree of truth it might in reality be capable of upon its firſt production and adjuſtment, is not now to be aſcertained, he never having uſed it for the graduation of any capital inſtrument. Thoſe that he made with a view to an accurate meaſure of angles, he always made with a ſcrew and wheel, or parts of circles cut by his engine into teeth, and ground together as before-mentioned; but I have reaſon to think, that its performance, if put to a ſtrict teſt, was never capable of that accuracy that he himſelf ſuppoſed it to have.

The method itſelf, however, from its ſimplicity, and eaſe of execution, ſeems to me to be a foundation for every thing that can be expected in truth of graduation; and in conſequence for reducing inſtruments to the leaſt ſize that is capable of bringing out all that can be expected from the largeſt; when it ſhall, like manual diviſion, have received thoſe advantages that the joint labours of the moſt ingenious men can beſtow upon it. That I may not appear to be without grounds for my expectations, I will beg leave to propoſe, what near forty years occaſional contemplation has ſuggeſted to me on the ſubject; and as I can deſcribe the proceſs I would purſue, where different from HINDLEY's, in fewer words than I could make out a regular criticiſm upon his letters, I will immediately proceed to the deſcription of it.

Proposed Improvements *of* HINDLEY's *Method.*

I would recommend the number of parts into which the circle is to be reduced to be 1440; that is 4 times 360; which diviſions will therefore be quarters of a degree; the diſtances of the holes in the chaps will therefore, to a three-feet radius, be $\frac{1}{1000}$ of an inch nearly; that is, between the one-ſixth and one-ſeventh of an inch diſtance centre and centre.

Having

Having provided myſelf with a ſtout mandrel, or arbor, for a *chock lathe*, properly framed, that would turn a circle of ſix feet diameter, I would prepare a chock, or platform, for the end of it, of that diameter, or a little more, compoſed of clean-grained mahogany plank, all cut out of the ſame log; which, when finiſhed, to be about $1\frac{1}{2}$ inch thick, and formed in ſectors of circles, ſuppoſe 16 to make the circle; the middle line of each ſector lying in the direction of the grain of the wood, this will conſequently every where point outward: the method of framing this kind of work is well known.

The way of getting a ſlip of braſs to anſwer the circumference of this platform is ſuggeſted in Mr. BIRD's account of conſtructing mural quadrants. Let a parallelogram of braſs of about three feet long, and of a competent ſubſtance (ſuppoſe half an inch) to make it when finiſhed about one-twentieth of an inch in thickneſs, be caſt of the fineſt braſs; and this to be rolled down till it becomes of ſufficient length for the hoop, and about one-fifth part more. I would then cut off, from the whole length, ſomewhat better than one-ſixth part, the whole being ſufficiently reduced to a thickneſs by the rollers. Perhaps no way will be more ready and convenient to get ſuch a long ſtrip of braſs reduced to an equal breadth, than the method preſcribed by HINDLEY; viz. by turning it upon the chock prepared; but I would not make it wider on one ſide than the other, like the hoop of a caſk, as he deſcribes, but exactly to fit the chock, when truly cylindric; for the internal elaſticity of the braſs, in ſo great a length, will be very ſufficient for fitting it on tight enough, without any tapering. This I will now ſuppoſe done; and a pair of ſteel *chaps*, as deſcribed by HINDLEY, to be alſo prepared, and ready for grinding; which, by ſuch a careful admeaſurement as can eaſily be made, will give the length of the hoop ſufficiently near, on its firſt preparation.

Method of forming a Pair of Straps as a Check to the Diviſions.

The part firſt cut off muſt be again cut into two equal parts in length; which, for diſtinction ſake, I will call the *straps;* and which are to ſerve as checks to every 60th and every 120th diviſion of the circle.

A ſteel plate, of about half an inch in breadth, the ſame thickneſs as the ſtraps, and in length equal to the breadth of the hoop plate, muſt be ſoldered with ſilver ſolder to one end of each of the ſtraps, by which means their length will be increaſed half an inch by the ſteel. An hole muſt then be made through each ſteel plate, of the ſame ſize as thoſe through the chaps, and anſwerable to the middle of the ſtraps; but

ſo

ſo near the border of the ſteel, that when the chaps are put on, and adapted to the ſteel hole, the next will fall through the braſs. The ſteel plates muſt then be hardened; and a pin being put through the two holes and the two plates, theſe muſt be wrought to a right line in contiguity to each other; by this means the ſtraight edge of each of the ſtraps will be reduced to the ſame diſtance from the ſteel hole: the hard ſteel edges may be rectified by the grindſtone, if neceſſary.

This being done, not only the holes in the chaps, but the holes in the two ſteel plates, applied to each other, like the two ſides of the chaps, muſt be reſpectively ground together; not with a taper pin, as preſcribed by HINDLEY; but ſo as not only to be cylindrical, but that the ſame cylindrical pin ſhall equally fit them all, and leave them ſmooth and poliſhed; which is a proceſs no ways difficult to a curious artiſt, and of which therefore a minute deſcription is unneceſſary.

The chaps being then put upon one of the ſtraps, with its ſtraight edge uppermoſt, and a pin put through the holes on the left-hand, and through the ſteel hole in the ſtrap under operation, the chaps muſt be ſet upright, ſo that the line joining the centres of the holes ſhall be parallel to the upper edge of the ſtrap; the braſs plate mentioned by HINDLEY, between the chaps, as a guide for directing them always to that upright poſition, may be then adjuſted and fixed to the inſide of the chap next the operator *.

The performance of the enſuing part of this work ſhould be at a ſeaſon when the temper of the air is not very variable; rather above the mean temper (ſuppoſe 60°) than below it; but above all things the artiſt ſhould be himſelf cool; that is, not in a ſtate of ſenſible perſpiration; and there ſhould be a free circulation of air in the room. Things being thus conditioned in reſpect to temperature, he may begin to drill the holes in one of the ſtraps; the pin being firſt put through the chaps and through the ſteel hole of the ſtrap; and the next hole, being drilled through the braſs with a common drill, that and every hole as it goes is to be finiſhed with a taper broach, as preſcribed by HINDLEY; and he may then prove or finiſh every hole by the ap-

* It would be well, previous to the drilling of the steel chaps, that another hole was drilled in the chaps, that should be somewhat above the upper edge of the straps, and in the middle betwixt side and side, to receive a *steady pin* therein, antecedent to drilling the main holes; for then a tempered steel pin, a little taper, will, by driving it in as far as necessary, constantly answer this purpose from first to last, so as to regulate the holes in grinding, to be truly opposite: proper holes should also be drilled for fixing the brass guide plate to one of the chaps.

plication

plication of a thorough broach, made ſo full as to require a degree of preſſure to force it through; and this broach being a little tempered, and the holes quite hard, there will be no fear of injuring the ſteel holes *.

Calling the hole in the ſteel plates o, and obſerving the time of beginning, you may proceed to drill 60 holes as preſcribed by HINDLEY; and noting how long you have been about it, you may lay the work aſide the length of time, equal to the time you took in drilling; that any addition of warmth it may have acquired in handling or working may be again loſt in a great degree †. After this pauſe you may begin again, and go on to finiſh 60 holes more; that is, to the length of 120 holes from the beginning; you then proceed in the ſame manner with the other ſtrap.

Method of drilling *the Hoop.*

You are now prepared to commence the work upon the long or *hoop-plate;* and you proceed therewith, in forming the firſt hole with the chaps, as before directed by HINDLEY, and this firſt hole you call o. You then place the ſtraps one on each ſide the hoop, with their gaged edges upward, and put the pin through the holes denominated 60 upon the ſtraps, and through the firſt hole already made, and denominated o upon the hoop; then, bringing the gaged edges of the ſteel plates to be even with the upper or working ſide of the hoop, you pinch them together in the viſe, and drill and broach the hole through the ſteel plates, which will make the hole, number 60, upon the hoop. This done, you put the pin through the left-hand hole of the chaps, and the hole marked o upon the hoop-plate firſt made, and proceed to drill with the chaps to 59 holes incluſive, which will fill up the whole ſpace from o to the 60th diviſion before obtained.

* The ſteel holes in the chaps need not to be above one-twentieth of an inch in diameter; and though it may be proper to make the ſteel plate, of which they are formed, one-tenth of an inch thick, in order to give the ſpring formed between them a convenient degree of ſtiffneſs, yet they may be reduced (by chamfering the outſides) to half that thickneſs.

† As there is not much occaſion for the artiſt to touch his work, the effects of that may alſo be very much avoided by wearing thick gloves; and the friction being but ſlight, and the work almoſt continually in the viſe, the variation of temperature in the metals concerned cannot be ſenſible or conſiderable.

You now again have recourſe to the ſtraps, and placing them one on each ſide the hoop-plate, you put the pin through the 120th hole of the ſtraps, and through the hole marked o upon the hoop-plate; and regulating the ſteel plates to the hoop-plate as before, you drill and form a hole with the ſteel plates, which will correſpond with the 120th hole upon the hoop-plate; and afterwards filling up the 59 holes wanting, by means of the chaps, you then have all completed to the 120th diviſion, which is one-twelfth of the whole circle.

You then proceed, in like manner, with another ſet of 120 holes; that is, placing the 60th hole of the ſtraps to the 120th hole of the hoop-plate, and from it producing the 180th hole; you, in like manner as before, fill up this 60 with the chaps; and afterwards placing the 120th hole on the ſtraps in the 120th hole on the hoop-plate, you will obtain the 240th hole; ſo that filling up this laſt ſet of 60 diviſions, you have obtained 241 holes, including 240 ſpaces or diviſions of the hoop; and repeating this proceſs ten times more, you will, in like manner, obtain 1441 holes, comprehending 1440 ſpaces *. And this proceſs being carried on in temperate weather, the manner of working produces twelve ſimilar operations, wherein the materials and tools concerned will not only be ſubject to very little change of temperature, but that change, whatever it is, will be nearly ſimilar in each ſet of 120 holes: we may therefore infer, that the greateſt inequality, or indeed any that can be ſenſible, muſt be at every 60 diviſions, that is, between the 59th and 60th, and between the 119th and 120th, both which will be equally repeated 12 times, in the whole length which is to compoſe the *circumference of a circle*, and which will thus be checked thereby 12 times in the circumference, and 12 times more at the intermediate diſtances; that is, with 12 maſter checks, and 12 ſubordinate ones, in the whole round.

It is proper here to obſerve, that in M. ROEMER's method even ſixty diviſions could ſcarcely be truſted in an affair of great accuracy, on account of the objections already made, ariſing from the points having ſuch ſlight hold in the ſurface of the braſs; but here the parts are held ſo exceedingly firm, and the operation carried on with ſo much power, that any ſmall inequality in the hardneſs of the braſs, or irregularity of ſurface,

* It will be proper, for reaſons hereafter to be mentioned, to continue the diviſions to 20 holes more, making in the whole 1461 holes.

cannot

cannot be ſuppoſed to affect the place of the centre of the hole; nor will any ſmall inequality that may be ſuſpected from the wear of the ſteel holes ſenſibly affect the *centre* of the hole, to which every thing is ultimately referred.

Method of joining *the Hoop.*

A more happy thought than that of HINDLEY's, for joining the two ends of the hoop, could ſcarcely have been wiſhed for, in regard to preſerving the ſame equality of the ſpace between the holes contiguous to the joint, as in the other parts: for though, geometrically ſpeaking, the two *saddle* plates, in which the little cylindrical bolts are fixed, for bringing the terminating holes of the hoop plate to their due diſtance, being one applied within the hoop, and the other without, will belong to circles of different *radii*; yet this difference being exceedingly ſmall in ſuch thin metal, and ſo great a radius, and one being as much too big for the hoop as the other is too little, when the bolts are put in, and the hoop in that part ſet nearly to a circle by a mould; the mean between them aſſumed by the hoop, from the elaſtic compreſſibility of the materials, will be the truth.

It muſt, however, be remarked, that in the uſe of the ſtraps, the joining of the hoop ſhould not be made at any part betwixt an 119th and an 120th diviſion, as ſome inequality muſt be ſuppoſed there, unleſs the ſaddle plates were adapted thereto. The method the moſt eaſily practiſed, will be to continue the diviſion upon the hoop, about twenty more than the completion of the number intended to form the circle, and to cut off all the overplus ones at the beginning.

The ſaddle plates I would recommend to contain ten holes each; ſo that if the diviſions are carried on to twenty more than what will be contained in the circle, there will be a piece containing twenty to cut off; and this again being cut in the middle will afford ten holes to make each ſaddle plate; ſo that there will be a place for a bolt on each ſide the joint, and then putting a bolt through every other hole, there will be three bolts at an end.

The pieces deſtined for the ſaddle plates, thus obtained, being broader than can be admitted when put to this uſe, I would adviſe to divide the breadth of the plate into three equal parts; and with a cutting hook (which perhaps will be attended with the leaſt violence in the ſeparation) to ſeparate the two outſide pieces from the middle piece:

by

by this means the two ſaddle plates (though double) will occupy one-third only of the breadth of the hoop in the middle; and two of the pieces cut off being applied, one on each ſide of the ſaddle plate on the outſide, will anſwer in like manner for the *rivet* plates.

The laſt operation to complete the joining of the hoop is the putting on the rivet plates: to complete this, I would adviſe a piece of braſs, of three or four inches in length, to be filed ſo as to anſwer to the inſide of the hoop, when reduced to a true circular form; and being three-eighths, or one-half an inch in thickneſs, to file the oppoſite ſide ſomewhat nearly concentric thereto; apply the middle of its convex arch to the inſide of the hoop at the joint, and then bring on the middle of one of the rivet plates to the joint of the hoop, confine the three together by a couple of narrow-chapped hand viſes, leaving a ſpace between them capable of receiving a couple of pins as rivets on each ſide the joint; the holes for the rivets are then to be drilled through all, and a little ſmoothed with a broach at their entry, into which ſmooth taper pins are to be driven; not with violence, but moderately, that no ſenſible ſtretching of the ſolid parts may take place thereby; then cutting off and ſmoothing the heads, ſhift the viſes ſo as to receive another couple of holes, and a third couple in the ſame end of the hoop; and proceed progreſſively in the ſame manner, from the middle to the other end of the rivet plate; then gently ſeparate the internal braſs mould with a thin knife, or ſuch like inſtrument; and cutting off, and very lightly rivetting the inner ends, proceed to fix the other rivet plate, in the ſame manner, on the other ſide: by this means the hoop will be firmly joined in the very poſition given it by the ſaddle plates and mould. Theſe plates may then be removed, the inſide of the hoop cleared and ſmoothed, if neceſſary; and the outſide will have the middle part clear where the diviſions lie, and that without ſenſible loſs or gain in the juncture.

Method of transferring *the Diviſions of the Hoop to a dividing Plate.*

The hoop being thus refitted for the chock, that ſhould be turned down to leave a ſhoulder on one ſide, that the hoop, now reduced to an equal breadth, may be forced againſt it; and the diviſions, being equally diſtant from one of its edges, will be all found in a circle, as if turned upon it. It ſhould be very carefully fitted to the chock, that it may go on with a ſufficient degree of tightneſs, and without the neceſſity of much forcing; and it will be no inconvenience now, if it goes on upon a very ſlight degree

degree of taper of the chock, as the internal ſpring of the materials will eaſily accommodate it to this ſhape without any injury to its general truth: a ſlight degree of a groove ſhould be turned in the place where the diviſions will come, that any conical pin, that is to ſerve as an index, let drop into the diviſions or holes, may not, by reaching through this thin plate, abut upon the wood, rather than upon the ſides of the holes; and thus this hoop is made into a wheel of 1440 equal diviſions, moveable round upon its own axis, whereon it was formed.

Againſt the time that this is completed, there muſt be prepared a flat circular plate or wheel of braſs, the rim of which ſhould be of about $3\frac{1}{2}$ inches breadth, and about two-tenths of an inch in thickneſs when finiſhed, to make a dividing plate; the external diameter of this is to be ſuch, that when laid flat upon the ſurface of the mahogany platform, its extreme edge will exceed the diameter of the hoop by about half an inch all round. There muſt alſo be prepared braſs arms (ſuppoſe eight in number) of an equal ſubſtance with the outer rim, and all connected with a circular plate in the middle; and, the whole of this work being framed beforehand, is to be let on flat upon the mahogany platform; whoſe face is ſuppoſed to be turned truly flat, and ſufficiently affixed with ſcrews: in this ſituation, the outward edge is to be turned, and the outward face of the rim turned flat. The *centre plate*, which may be about twelve inches diameter, is alſo to be turned as flat as poſſible, and a centre hole, of about half an inch diameter, to be very carefully turned therein.

A piece of clean, ſtraight-grained, well-ſeaſoned mahogany, of about two feet long, three inches thick, and five or ſix inches broad, is then to be well affixed to ſome part of the general frame of the lathe, which muſt now have its poſition altered, ſo that the platform will become horizontal; and therefore the frame ſhould be originally made with this view*. The piece of mahogany is to be affixed ſo that one of its larger faces ſhall be in a parallel plane to the face of the platform, and ſo low as to clear the under ſide of the platform in its rotation; and ſo far diſtant from the centre, that an index may be fixed upon this upper face of the piece of wood, ſo as conveniently to drop into the holes of the hoop; while the common *cutter frame* of a clock-maker's engine ſhall be firmly attached upon the ſame face of the wood, and ſo

* After changing the poſition of the lathe, the collar of its mandrel ſhould be removed, and the neck made to move within three planes, ſo as to preſerve an exact centre, in the manner of an *equal altitude* inſtrument.

fixed

fixed as to cut the edge of the dividing plate into teeth, anſwerable to the ſeveral diviſions of the hoop. The teeth need only to be cut with a common cutter, making a parallel notch: and here it will be proper to obſerve, that not only both the index and cutter are to be founded on the ſame piece or baſe of wood; but that the nearer they are together, the more free they will be from the effects of all variations of expanſions by variations of temperature *.

The equalising the Teeth of the dividing Plate by grinding.

The object of transferring the diviſions of the hoop to the teeth of the dividing plate, is ſtill farther to equalize the teeth by grinding; eſpecially thoſe that, falling within the compaſs of each ſet of 120 diviſions, may be ſuppoſed, if any, to be mended thereby; but as it may be incommodious to conſtruct a curved ſcrew, of ſuch a length and ſize, in HINDLEY'S method, as would be ſufficient for the purpoſe; I would propoſe to uſe two ſcrews of braſs, cut from a cylinder in the way ſet forth by Mr. RAMSDEN, each of which, with a very little grinding upon this large circumference, would lay hold of ten or twelve teeth together. I would place the two ſcrews, that is, their middles, to be ninety diviſions aſunder; of conſequence, when one of the ſcrews is between the 59th and the 60th, or between the 119th and 120th diviſion of each ſet, the other will be in the middle of the ſpace divided by the chaps only †.

The threads of theſe ſcrews I would adviſe to be cut a little taper, ſo that as they grind in, they may fill the notches of the teeth; which alſo, by this means will acquire a little tapering towards their extremities; and by cutting the notches parallel,

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* It is proper to obſerve, that as it may be impracticable to get the rim of the dividing plate caſt of the proper ſize, in one entire piece, it will be very practicable, if caſt of a leſs ſize (ſuppoſe half), but of a ſufficient thickneſs, to roll it down; and by having the outward edge originally thicker than the inner, in the proportion of the *radii*, it may be ſo managed by the rollers as to be of an equal thickneſs when brought to its proper ſize. But the arms and centre plate ſhould be of the ſame metal, rolled in the ſame degree.

† The beſt way of giving an equal motion to thoſe two ſcrews, ſeems to be by a detached axis carrying two common flat wheels; one acting upon a like flat wheel, upon the axis of one ſcrew, and the other, in the ſame manner, upon the other; and applying the pulley for communicating the power to the middle of the detached axis between the two wheels, the ſpring or twiſt will be equal both ways; ſo that in turning the contrary way round, they will ſtill be in equal advance.

as I have mentioned, the true ground part will always be certain of being at the extremity.

When the ſcrews have been uſed in grinding till they are found to have the effect of a perfectly equal and eaſy rotation all round, and all the teeth reduced to a ſenſible taper, and regular bearing, I would then totally remove the ſcrews from the ſquare block of wood, upon whoſe upper face I ſuppoſe them to have been mounted; in like manner as I ſuppoſe the index and cutting frame to have been removed, to make room for the mounting of the ſcrews. I now conſider the teeth of the dividing plate, ſo formed, as having all the equality that the preſent known ſtate of human art has pointed out; and the whole convertible upon the axis or mandrel upon which it has been originally formed, and the central hole of the plate concentric therewith: I therefore conſider the ground faces of the teeth of the plate as the actual diviſions. It now remains to ſhew how they are to be transferred, to form the diviſions of an inſtrument.

Preparation *of the dividing Plate for* graduating *Inſtruments.*

If a ſmall cylinder of hard ſteel is duly poliſhed, and made of a ſize ſo as juſt to chock in betwixt the extremities of the teeth, then the centre of that cylinder will be a fixed point, in reſpect to the circumference of the wheel: if another cylinder is applied in like manner, at the diſtance of a number of diviſions (ſuppoſe it a prime number, ſo as to croſs all former diviſions, viz. 17 or 19), then the middle of the line joining the centres of the two cylinders will remain in the direction of the *ſame radius*, though one of them ſhould force in a minute quantity further than the other; and if a point is aſſumed in the direction of a tangent to a circle at this middle point, then though both the cylinders ſhould drop in a minute quantity further at one time than another, yet the middle point would remain at the ſame diſtance from the point in the tangent; provided that point was removed to a competent diſtance, that is, to five or ſix inches. On this principle I would conſtruct an index, the two cylinders being fixed in a frame, convertible about the middle point, and to be centered in the end of the lever, repreſenting the tangent; then this lever being again convertible about the point in the tangent line, the middle point would always have a fixed diſtance from the point in the tangent, and there hold it ſteadily faſt; the tangent point being placed upon the fixed block beforementioned.

Uſe

Uſe *of the dividing Plate in the* Graduation *of Inſtruments.*

Our dividing plate is now ready for the reception of an inſtrument; ſuppoſe it a quadrant, whoſe radius, however, muſt not exceed the radius of the dividing plate: It is to be laid upon the face of the dividing plate, and a weight, or weights, equivalent to that of the quadrant, is placed on the oppoſite ſide, to balance it. It muſt alſo be ſuppoſed, that the quadrant is made with a view to be divided by this engine; and conſequently, that the central cylinder is ſo well adapted, and nicely fitted to the centre hole of the quadrant, that the centre cylinder can be removed, in order for the limb to be divided, and again replaced; without ſenſibly altering its centre. This being the caſe, let a piece of metal be turned, to apply to the quadrant, perfectly like its centre cylinder at the upper end, and turned nicely to fit the central hole in the dividing plate, at the lower end; then, the quadrant being fixed with proper faſtening ſcrews, I would cut the diviſions with a beam compaſs; and, if a fixed point is aſſumed, viz. the centre of the tangent point for the index; then the beam compaſs being always opened to the computed length of the tangent of the circle of diviſions, it will be ſufficiently near for cutting the diviſions, ſquare to the circular arches between which they are placed.

It will alſo be proper (to prevent unequal expanſions) that the beam of the compaſs ſhould be formed of a piece of clean-grained *white fir*; and that the length between the points be incloſed in a tube of tin or braſs; without touching the beam, except at the terminations, which will in a great meaſure protect it from both alteration of moiſture, and of heat from the body of the artiſt, during the operation.

It will be likewiſe proper to have a lever, or ſome equivalent contrivance, to bring the dividing plate forward; that after lifting the little cylinders out of the diviſions, and reſting them upon the tops of the teeth, they may be brought gently forward with an equal drag, and ultimately ſnap in between the teeth, by the ſtrength of the ſpring commanding the index; by this means the drag of the friction of the whole will be conſtantly the ſame way.

Conclusion.

Now, if, as it has been ſhewn, a quadrant of any radius may be read off to the 4000dth part of an inch, then this quantity upon a radius of three feet will not be ſo much

much as $1\frac{1}{7}$ ſecond; and as the whole of the proceſs is carried on by contact, in which a greater error than that of a 60,000th part of an inch cannot be admitted in any ſingle operation, I ſhould aſſuredly expect a three-feet quadrant, ſo divided, to be true in its diviſions, and read off to at moſt two ſeconds.

But, after all, in an inſtrument like this, I ſhould expect the greateſt ſource of error to be in the want of perfect coincidence of the centre of the diviſions with the actual centre upon which the index revolves; and therefore, that if, inſtead of a quadrant of three-feet radius, a complete circle of five feet diameter was divided, and its diviſions read off from the two oppoſite points (taking the mean), then the errors of the centre will be wholly avoided. For this reaſon, I am very clearly of opinion, that the ſagacious propoſition of Mr. RAMSDEN, to uſe circles inſtead of quadrants, or other portions of circles, will bid much the faireſt for perfection in actual practice; and that his ingenious method of making them both ſtiff and light, by the uſe of hollow conical tubes by way of ſpokes, in the manner of a common wheel, will enable him to mount them of five feet diameter, upon hollow axes, in the nature of a *transit*. By this means we ſhall have all the good properties of both the quadrant and tranſit united in one inſtrument; and obſervations both of right aſcenſion and declination, through the very ſame teleſcope, as long ſince attempted by M. ROEMER; and to a degree of perfection and certainty, in point of declination, hitherto unattainable by the largeſt inſtruments that have yet been made.

N. B. In matters of very nice determination, ſmall circumſtances often come to be of conſequence; and it is in this view that I mention what follows. It was a practice of HINDLEY's of many years ſtanding, and ſince followed by myſelf and others, wherever he made any uſe of the *vernier*, to lay the vernier plate in the ſame plane, or cylindrical ſurface continued, whereon the principal diviſions are cut. It is of equal utility, though the vernier be rejected, to lay the index ſtroke in the plane of the diviſions. In this way the diviſions being by convenience upon the external border of the limb *, *two* ſets of diviſions are thereby rendered incommodious; but thoſe that

* It has been objected, that laying the divisions upon the extreme edge of the limb of the instrument subjects it to injury: but, to obviate this, in an HADLEY's quadrant made for me, by my direction, by the late Mr. MORGAN, in the year 1756, wherein the vernier is laid even with the divisions; those are protected by a projection of the solid part of the limb, beyond the divisions; a *Rabbet* being sunk in the edge of the limb, to clear the vernier.

wiſh two ſets, as a check, will in a great meaſure aid themſelves, by reading from two different parts of the ſame ſet of diviſions; which is very eaſily provided for, by putting an additional ſtroke upon the index plate, at the diſtance of 9, 11, or any prime number of diviſions to 19, 23, or more; and reading off from that ſtroke alſo; as before recommended for great quadrants, where the vernier is propoſed to be rejected *: ſo that they will thereby be mutually checked by diviſions that had no correſpondence in their original formation.

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* I would not have it thought, from my proposal of rejecting the vernier, that I have any quarrel with it; I think it a very simple and ingenious contrivance, where it is properly applicable; that is, where the strokes of the vernier, or their estimated halves, are sufficient for all the precision required or expected from the instrument, as in HADLEY's quadrants, theodolites, &c. but where still more minute divisions are required than can easily be had by estimation from the vernier; to do this by a screw, as a *supplement* to the vernier, appears to me in the light of bringing a more accurate tool to supply the deficiencies of one less accurate; when the former might, with more propriety, supply the place of the latter altogether.

REMARKS

REMARKS on the different Temperature of the Air at Edystone, from that observed at Plymouth, between the 7th and 14th of July, 1757, by Mr. JOHN SMEATON, F. R. S.

Read January 12th, 1758.

SIR,

ON reading of Dr. HUXAM's letter at the laſt meeting, ſome obſervations occurred to me, concerning the different temperature of the air, which I had obſerved at the Edyſtone, from what had been obſerved by the Doctor at Plymouth, between the 7th and 14th of July laſt; which, having been deſired by ſome members to be put into writing, I beg leave to trouble you with the following:

Edyſtone is diſtant from Plymouth about ſixteen miles, and without the head-lands of the ſound about eleven.

The 7th and 8th were not remarkable at Edyſtone for cold or heat; the weather was very moderate with a light breeze at eaſt, which allowed us to work upon the rock both days when the tide ſerved.

About midnight, between the 8th and 9th, the wind being then freſh at eaſt, it was remarkably cold for the ſeaſon, as I had more particular occaſion to obſerve, on account of a ſhip that was caſt away upon the rocks.

The wind continued cold the 9th all day, which was complained of by ſome of the ſhipwrecked ſeamen, who had not time to ſave their clothes; and ſo freſh at eaſt as prevented our going near the rocks, or the wreck; and ſo continued till Sunday the 10th; when ſeeing no proſpect of a ſudden alteration of weather, I returned to Plymouth in a ſailing boat, wrapped up in my thick coat. As ſoon as we got within the head-lands, I could perceive the wind blow conſiderably warmer, but not ſo warm as to make my great coat uneaſy. Having had a quick paſſage, in this manner I went home, to the great aſtoniſhment of the family to ſee me ſo wrapped up, when they were complaining of the exceſſive heat: and indeed it was not long before I had reaſon to join in their opinion.

This

This heat I experienced till Tueſday the 12th, when I again went off to ſea, where I found the air very temperate, rather cool than warm, and ſo continued till Thurſday, the 14th.

In my journal for Wedneſday the 13th, I find the following remarks, viz. "This evening's tide," (from 6 A. till 12 A.) "the wind at eaſt, but moderate, with frequent flaſhes of lightning to the ſouthward. Soon after we got on board the ſtore veſſel, a ſquall of wind aroſe from the ſouth-weſt on a ſudden, and continued about a minute, part of which time it blew ſo hard, we expected the maſts to go by the board; after which it was perfectly calm, and preſently after a breeze returned from the eaſt."

And in the journal of the 14th is entered, "This morning's tide," (viz. from 1 M. to 1 A.) "the air and ſea quite calm."

Hence it appears how different the temperature of the air may be in a ſmall diſtance; and to what ſmall ſpaces ſqualls of wind are ſometimes confined.

It may not be amiſs further to obſerve upon this head, that once, in returning from Edyſtone, having got within about two miles of the Ramhead, we were becalmed; and here we rolled about for at leaſt four hours; and yet at the ſame time ſaw veſſels not above a league from us, going out of Plymouth Sound, with a freſh of wind, whoſe direction was towards us, as we could obſerve by the trim of their ſails; and as we ourſelves experienced, after we got into it by tacking and rowing.

I am,
SIR,
Your moſt humble Servant,
J. SMEATON.

Furnival's Inn Court,
12th Jan. 1758.

AN

AN ACCOUNT of the Effects of Lightning upon the Steeple and Church of Lestwithiel, Cornwall; in a Letter to the Right Honourable the Earl of MACCLESFIELD, President of the Royal Society, by Mr. JOHN SMEATON, F. R. S.

Read April 21ſt, 1757.

JANUARY 25th, 1757, about five o'clock in the evening, returning home from the Edyſtone works, near Plymouth, I obſerved four flaſhes of lightning, within the ſpace of ſix or ſeven minutes, towards the weſt, but heard no noiſe of thunder, being diſtant thrty miles. A fews days after I was informed that the ſame evening the lightning had ſhattered the church of Leſtwithiel in a very ſurpriſing manner.

The 1ſt of March I was at Leſtwithiel; they had then begun to repair the damages; but had not made ſuch a progreſs but that the principal effects were equally obſervable as at firſt I obſerved, and was informed as follows:

At the time before-mentioned, the inhabitants were alarmed by a violent flaſh of lightning, accompanied with thunder ſo ſudden, loud, and dreadful, that every one thought the houſe he was in was falling upon him; almoſt every one being within doors, on account of a violent ſhower of rain, which preceded the lightning; ſo that nobody ſaw or heard any thing of the miſchief done to the church, till it was obſerved accidentally after the ſhower.

The ſteeple is carried up, plain and ſquare, to about forty-nine feet, with a kind of ſlate ſtone, rough caſted on the outſide; upon which is formed a very elegant octagon gothic lanthorn, about nine feet high, and thereon a ſtone ſpire about fifty-two feet height, with a ſpindle and vane riſing about three feet above the ſtone; ſo that the whole together was about one hundred and thirteen feet. Each face of the lanthorn finiſhes above with a ſort of a gothic pediment, with a little pinacle upon each, ſeparated from the body of the ſpire.

I will not affirm, that the lightning entered in at the ſpindle or vane at the top, but will ſuppoſe it, for the ſake of methodizing the facts. The vane was of copper-plate, which, being turned round, and rivetted, made a ſocket to turn upon. The ſpindle did not reach through the ſocket, but the weight of the vane reſted upon the top of the ſpindle, the top of the ſocket being cloſed. About the vane were many acute angles, and ſome almoſt ſharp; but I did not obſerve any pointing directly upward. The vane

was

was much bruiſed, which might be occaſioned by the fall; but the ſocket was rent open as if it had been burſt by gun-powder, and in ſuch a manner as I cannot conceive could be occaſioned by the fall. Under the ſpindle, that carried the vane, was a bar about four feet long and one inch ſquare, that paſſed through the centre of ſeveral of the uppermoſt ſtones ſucceſſively, in order to unite them more firmly together, and was run in with lead; all which ſurrounding ſtones were broke off, except one, which, together with the bar, fell down within the tower.

The ſhell of the ſpire, as far down as thirty-five feet from the top, was no more than ſeven inches thick, and the courſes about the ſame heighth; ſo that ſcarce any one ſtone in the ſpire could weigh more than thirty or forty pounds; but they were joined together at the ends with mortoiſe and tenon, in a curious manner. Above twenty feet of the upper part was entirely thrown down, and diſperſed in all directions; and, as I was informed, ſome pieces were found at the diſtance of two hundred yards. A great many ſtones fell upon the roof of the church; and ſeveral made their way through both roof and ceiling down into the church, breaking the pews and whatſoever they fell upon. Six feet ſtill lower the ſpire was ſeparated, the weſternmoſt half being thrown down; the eaſtern half was left ſtanding, but disjointed, and in ſo critical a poſture, that it ſeemed ready to fall every moment, ſo that this was ordered to be taken down immediately; and likewiſe to ſix feet below, the work being found remarkably ſhattered. In this condition it was when I ſaw it. The whole of the ſpire I found much cracked and damaged, but the remainder of the ſeven inch ſhell ſo greatly, that there ſeemed ſcarcely a whole joint.

The pediments over every face of the lanthorn were damaged more or leſs, but the whole aſhlering of that to the N. W. was torn off from the inner wall; to which it was connected. At firſt ſight this might ſeem to be done by the falling of the ſtones from above; but I was convinced to the contrary, by obſerving, that ſeveral of the pediments were damaged, and even ſtones ſtruck out, where the little pinacles above them were left ſtanding.

About the top of the lanthorn is a bell for the clock to ſtrike on: it is hung upon a croſs-bar, with gudgeons at each end; the whole being ſuſpended to a beam laid acroſs the tower. The croſs bar was ſo bent, that the clock hammer could not touch the bell by above two inches. This could not be done by falling of ſtones, becauſe the beam would defend the bell from receiving any ſtroke in the direction to which the croſs bar was bent. As to the wire that drew the hammer, as I was informed, not one bit of it could be found.

The

The bells (four in number) for ringing, hung in the ſquare part of the tower, below the lanthorn, two above and two below: the wheels of every one were broke to pieces, and one of the iron ſtraps, by which they are faſtened to the yoke, unhooked, and as appeared to me could not be replaced without great force or unlooſing. Whether theſe accidents were occaſioned by the lightning or the falling of ſtones I leave undetermined. In the floor under the bells was placed the clock, caſed up with ſlight boards. The verge, that carries the pallets, was bent downwards, as if a ten pound weight had fallen ten feet right upon it. The crutch, that lays hold of the pendulum, looked as if it had been cut off by a blunt tool, and heated by the blow, till it was coloured blue, at the place where it was cut. It turned at a right angle, and might be about $\frac{4}{10}$ of an inch broad by $\frac{2}{10}$ thick. As to the pendulum, which hung pretty near the wall, the upper part of the rod was ſtruck with ſuch violence againſt the wall, that a ſmart impreſſion thereof was made in the plaiſter: and near the upper part of the impreſſion appeared a ſhady ring, of a blackiſh colour, ſomething like as if a piſtol had been diſcharged of powder, and the muzzle held near the wall. The caſing of the boards round the clock remained unhurt.

In this ſtory, on the north and ſouth ſide, are two narrow windows or air loops; againſt the upper part of which, on the outſide, were fixed the timber dials belonging to the clock, both of which were blown off and broke to pieces, poſſibly by the fall: and not only that, but part of the ſtone jambs were broke out alſo, near to where the rod paſſed, that carried the hands. In this ſtory alſo was a ſort of window or air loop on the eaſt ſide, that had communicated with the church, but was ſtopped up with lath and plaiſter; alſo ſeveral putlock holes for the ſcaffolding, which had gone through the wall into the church, but were ſtopped up with ſtone, and plaiſtered over; all theſe were forced out into the church and the plaiſter torn from the wall.

In the ground ſtory of the tower or bellfrey, the ſouth and north entrances were ſhut with wooden doors. The upper part of the eaſtern entrance, that communicated with the church, was made up with lath and plaiſter; and before it, in the church, are ſeats raiſed one higher than another; ſo that the floor of the ſeats next the wall was half up the door way; conſequently the vacuity under the ſeats lay open to the bellfrey.

About the middle of the weſternmoſt ſide one of the paving ſtones, about one foot ſquare, and one and a half inch thick, was thrown up, and a hole pierced into the wall, rather below the level of the pavement, into which one might put three fingers, on the oppoſite ſide, the ſouth weſt angle of the middle buttreſs, which together is eight feet; but the hole was too rugged and crooked to put any thing through

through. Beſides this hole the wall was pierced in ſeveral places, and the plaiſter thrown off both within and without. One place within about four feet above the floor, was a hole of about fourteen inches ſquare, pierced ſix inches in the wall; and ſo nearly ſquare, that I enquired whether it had not been made by art, but was aſſured of the contrary.

The north and ſouth doors of the tower were both blown out, and broke in m an pieces. Many of the arch ſtones over both doors were disjointed and diſplaced: two of the ſtones making the jamb of the ſouth door were forced quite out, and one of them broken.

The vaulting of the eaſt door way was plaiſtered underneath: the plaiſter was ſprung from the ſtone in 30 or 40 places, like as if a bar of iron had been drove from above through the joints of the ſtone, and thereby forced off the plaiſter, with its end. The lath and plaiſter partition, which ſtopped up the upper part of this door-way, was forced into the church, and the wainſcotting making the back of the laſt ſeat was torn from the wall from end to end. Some part of the vapour ſeems to have made its way through the cavity under the ſeats; for moſt of the boards, compoſing the riſe of the ſteps from ſeat to ſeat, were blown out forwards; and ſeveral pannels of wainſcot at each end of the ſeats above mentioned, were forced out and broken. Hence, the vapour ſeems to have divided itſelf into three branches, one moving directly forward to the eaſt window, being 13 feet wide, and about 20 feet high, conſiſting of five principal lights divided by ſtone mullions: two of the lights were in a manner wholly deſtroyed, and ſeveral large holes in thoſe remaining; the glaſs and lead being carried outward, like as if an harlequin had leaped through the window. The north window, fronting the broken pannels of the ſeats, was very much ſhattered: but the ſouth window oppoſite had ſcarce a whole pane left.

It is farther to be noted, that almoſt all the lights in the church, though not broken, were bagged outward.

N. B. It was ſaid in the London Papers, that the organ was entirely ſpoiled: it is certain there is not, nor ever was, any organ in this church.

FINIS.

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